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A COMBAT WITH AN INFECTIVE ATMOSPHERE.[1]

By Professor JOHN TYNDALL, F. R. S.

A YEAR ago I had the honor of bringing before the members of the Royal Institution some account of an investigation in which an attempt was made to show that the power of atmospheric air to develop life in organic infusions—infusions, for instance, extracted from meat or vegetables—and its power to scatter light went hand-in-hand. I then endeavored to show you that atmospheric air, when left to itself, exercised a power of self-purification; that the dust and floating matter that we ordinarily see in it disappeared when the air was left perfectly tranquil; and that, when the air had thus purified itself, the power of scattering light and the power of generating life had disappeared together. For the sake of reminding you of this matter, we will now cause a beam of the lamp to pass through the air. You see the track of the beam vividly in the air. You know that the visibility of the track is not due to the air itself. If the floating matter were removed from the air, you would not be able to track the beam through the room at all. You see the track in consequence of the floating dust suspended in the air. If the air be inclosed in a place free from agitation the dust subsides, and then, as I endeavored to show you a year ago, the air possesses no power of generating life in organic infusions. The nature of the argument is this: You see the dust as plainly as if it were placed upon your hand, and you could feel it with your fingers. You found that the dust, when it sowed itself in organic infusions, produced a definite crop in those infusions; and you are equally justified in inferring that the crop thus produced is due to the germs in the dust, as a gardener would be in believing that a certain crop is produced from the seeds which he

[1] A lecture delivered at the Royal Institution, on Friday, January 19, 1877.

sows. I say that the inference that his crop is the product of the seeds that he sows is not more certain than the inference that those crops produced in the organic infusions are due to the seeds contained in them.

You know the method that we resorted to for the purpose of enabling us to get rid of this dust. The object was, to allow the air to purify itself, and it was done in this way: I have here the first chamber that was used in these experiments. You see at the bottom a series of test-tubes entering the chamber; they are air-tight, and they open into it. There are windows at the sides, and here is a pipette through which the liquids can be introduced. Behind we have a door which opens upon its hinges. Now, imagine this perfectly closed; imagine it abandoned entirely to itself, left perfectly quiet. In a few days, the floating dust of the air contained in the chamber entirely disappears—it has removed itself by its own subsidence—and then, when you send a beam of light such as we have here through these windows, you see no track of the beam within the chamber. When the air is in this condition, you pour through this pipette infusions of beef, mutton, or vegetables, into these tubes, and allow them to be acted upon by the air. Last year, between fifty and sixty of these chambers were constructed, and the invariable result was that these infusions never putrefied, never showed any change, were perfectly sweet months after they were placed there, as long as the air had this floating matter removed. You had nothing to do but to open the back-door and allow the dust-laden air to enter the chamber to cause these infusions to fall into a state of putrefaction, and swarm with microscopic life, in three days after opening the door. I have a smaller chamber here—for we use chambers of different sizes—and it will enable you to understand our exact process. (*See* Fig. 1.) You see here the stand on which the chamber rests. There are two bent tubes that communicate with the outer atmosphere, for I wish to have a free communication between the air outside and the air within. You see the pipette through which the tube is filled. When the infusion is poured in, you place it in an oil-bath contained in a copper vessel, such as we have here, in which you boil it for five minutes. Now, that boiling for five minutes was found capable of sterilizing every germ contained in the infusions placed in these chambers. This year our experiments began by a continuation of those that we made last year. In order to enable you to judge of the severity of the results obtained last year, I have here five cases belonging to the experiments then made. You will see that the infusions are vastly concentrated because of their slow evaporation. The quantity of liquid is reduced to one-fifth of its primitive volume, but this one-fifth is as clear as rock-crystal; whereas, the tubes exposed to the ordinary air outside fell long ago into utter putrefaction. They became turbid and covered with scum, and when you examine these

infusions to ascertain the cause of that turbidity, you find it to be produced by swarms of small active organisms.

This year our inquiries began in the month of September. But we will pass over these inquiries for the moment and go to those of October. On October 29th, two members of the Royal Institution collected a quantity of fungi in Heathfield Park, Sussex. These were

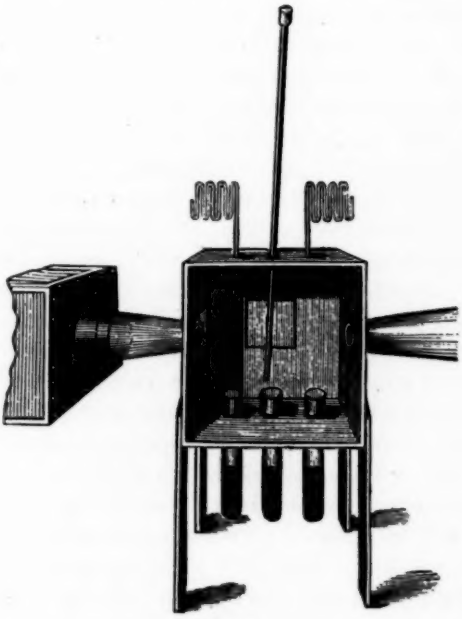

Fig. 1.

brought to London on the 30th. They were placed for three hours in warm water, and, whatever juices they possessed, were thus extracted from them. They were placed in chambers and digested separately. There were three kinds of fungi; we will call them red, yellow, and black. Now, I confess that, thinking I had secured a perfect freedom from any invasion of those contaminating organisms that produce putrefaction, I expected that we should find that these infusions of fungus would maintain themselves perfectly clear. To my surprise, in three days the whole of them broke down; they became turbid, and covered by a peculiar fatty, deeply indented, corrugated scum. Well, that was a result not expected, but I pursued the matter further. I got another supply of fungi. Even in this first experiment, I had used care at least as great as that which I adopted last year, and which led to a perfect immunity from the invasion of putrefaction. With the fresh supply of fungi, I operated with still more scrupulous care. The infusions were placed as before in three chambers. In one of these, the infusion remained perfectly pellucid;

there was no trace of any organism to be seen. In each of the other chambers one of the three tubes gave way. Each chamber contained three tubes; so that, out of nine tubes containing an infusion of fungus, seven proved to be intact, entirely uninvaded. Therefore, whatever argument or presumption was raised by the first chamber in regard to the idea that life was spontaneously generated in it, was entirely destroyed by the deportment of the other chambers. Seven out of the nine remaining intact, was sufficient to show that it was some defect in the experiment that caused the first chambers to give way so utterly. I continued the experiments, and, inasmuch as fungi disappeared on the approach of winter, other substances were chosen. I took cucumber and beet-root, having special theoretical reasons for doing so, and prepared infusions of them with the aid of my excellent assistant Mr. Cotterell. We placed these in our chambers as before, boiled them for five minutes, and abandoned them to what I supposed to be the moteless air within. Again, to my surprise, an infusion of beet-root in one chamber, and an infusion of cucumber in another, broke down. All the tubes became turbid and covered with this peculiar fatty scum. Other chambers were then tried. I had begun to suspect that we were operating in a contaminated atmosphere; that my infusions were in the midst of a pestilence which it was hardly possible to avoid. The consequence was, that I withdrew the preparation of the infusions from the laboratory down-stairs, and I went to one of the highest rooms in the Royal Institution, had the infusions prepared there, and introduced into the cases, which were afterward boiled in the laboratory below. There were a great number of these cases. The substances chosen were cucumber, beet-root, turnip, and parsnip. Great care was taken to have the infusions properly prepared, and to have them rendered as clear as possible. To give you an idea of the care taken, I may mention that the infusions of turnip and beet-root were passed through 24 layers of filtering-paper, and were thereby rendered clear; that the infusion of cucumber was passed through 120 layers of filtering-paper, and thereby rendered clear; and that the infusion of parsnip was passed through 300 layers of filtering-paper, and it was still opalescent. The suspended particles were so small that the filtering-paper had no power whatever to arrest them, and the finest microscope ever made would have proved powerless to exhibit the individual particles that produced this opalescence. Notwithstanding all this care, the chambers containing these infusions in three days became filled with bacterial life. They were turbid, covered with scum, and showed all evidences of putrefaction. This was on November 20th. On November 25th we went up-stairs and prepared another chamber, or a series of chambers. When the tubes containing the infusions were placed in the oil-bath, the liquids within the tubes opening into the case of course boiled, steam was discharged into the case, the air of the case being thereby rendered warm. It

was found that on the cessation of the ebullition, although the pipette was immediately plugged with cotton-wool, and the bent tubes also plugged with cotton-wool, still, in consequence of the contraction of the air within, there was a considerable indraught. Last year, we found invariably that the interposition of the cotton-wool entirely sifted this entering air so as to arrest any germs or seeds that it might contain. I thought, however, in this case, that the germs might be carried in by the suction when the air of the chamber contracted. In the former case, we operated after having filled the chamber with the infusion and boiled it in the laboratory; in this case, we took the additional precaution of boiling the infusion up-stairs, and taking care that it was properly plugged with cotton-wool. But here, again, notwithstanding this augmented care, the infusion utterly gave way, and showed those evidences of life that had distracted me previously. When I say distracted, it is not meant that I was in the least degree daunted or perplexed about it. I knew perfectly well that the matter would be probed by-and-by. On November 27th a new chamber was constructed containing cucumber and turnip. Particular care was taken with the stopping of the pipette, and also the bent tubes opening into the atmosphere. In one instance, about this time, it was noticed that the infusions in the tubes within the chamber opening into the moteless air, or at least what I supposed to be the moteless air, fell more rapidly into a state of putrefaction, became more rapidly covered with scum, than the tubes exposed in the air outside. When the tubes containing precisely the same infusion were exposed to the air outside, they were perfectly clear, while those within were turbid and covered with scum. This brought to my mind an experiment made the previous year with trays placed one above the other.

It was found that, when two trays were placed one above the other, although the upper tray had the whole air of the room for its germs to deposit themselves, the under tray was always in advance of the upper in the development of life. The reason was simply this: The air in the under tray was less agitated, and this floating matter had time slowly to sink in the infusions. There was no other solution possible than that, by some means or other, the germs had insinuated themselves into my chamber, and that these germs, sinking slowly through the unagitated air of the chamber, were able to produce the effect within in advance of the effect produced upon the openly-exposed tubes without. On November 27th I had a similar case, and also on November 30th, and on December 1st. The chambers were prepared and filled with all care, and yet the infusions broke down, became turbid, and were covered with scum. I then had a number of tubes filled with infusions, and sealed them hermetically. They were exposed in an oil-bath, and heated for a quarter of an hour to a temperature of 230° Fahr., for I wanted to see whether these effects were due to any germs of life in the infusions themselves. This superheated

cucumber-infusion was introduced into the chamber, and it was found that the superheating of the infusion did not even retard the development of life. In two days every tube of the chamber was swarming with bacteria. I then passed on to another system of experiment pursued last year, that is, the exposure of the infusions to air calcined by passing a voltaic current through platina-wire, so as to raise the wire to a state of incandescence. Such arrangements are here. We have underneath this shade two wires, and stretching from wire to wire we have a spiral of platinum. Passing a voltaic current through the spiral, it was found last year that five minutes of incandescence were sufficient entirely to sterilize and destroy all germs contained in this air, and to protect the infusions underneath from all contamination; the time of incandescence was doubled this year. The wire was raised as close to the point of fusion as possible; still, notwithstanding all this additional care, the infusions one and all gave way. I thought that there might be some defect in the construction of the apparatus. Here, you see, is an old broken apparatus containing infusions that have remained perfectly good since last year; but great pains were taken in having the apparatus of the most improved form. Still, notwithstanding all my efforts, the infusions broke down and became swarming with life. My attention was now very keenly arrested, and on December 1st I scrutinized more closely than ever I had done previously the entry of the infusions through the pipette-tube into the tubes opening into the chamber, and I noticed, at all events, a danger of minute air-bubbles being carried down along with the descending infusion. That caused me to adopt another mode of experiment; but, previously to this, I fell back upon some of the infusions found so easy to sterilize the previous year. I operated upon beef, mutton, pork, and herring infusions, and found that even such infusions, which with the most ordinary care were completely sterilized last year, and are preserved to the present hour intact like the others, all gave way.

How, then, are we to look at these things? Here are results totally different from those that we obtained last year. You may ask me, perhaps: "Why do you not loyally bow to the logic of facts and accept the conclusion to which those experiments apparently so clearly point? Why do you not regard them as a demonstration of the doctrine of spontaneous generation? Is there any other way of accounting for it than by a reference to this doctrine?" You may ask whether I was held back by prejudice from accepting this conclusion, whether I was held back by a love of consistency, or by the fear of being turned into ridicule and sneered at by those whom I ventured to oppose on a former occasion. Ladies and gentlemen, there is a title which I believe, as the generations pass, will, if the owners of the title are true to themselves, become more and more a title of honor—that is, the title of a man of science—and of that title I should

be utterly unworthy were I not prepared to trample all influences and motives such as those mentioned under foot, and were I not ready, did I conceive myself to be in error in what was brought before you last year, to avow here frankly and fully in your presence that error. I should be unworthy of the title of a scientific man if my spirit had not been so brought into this state of discipline as to be able to make such an avowal. Why, then, do I not accept those results as proving the doctrine of spontaneous generation? The celebrated argument of Hume comes into play here. When I looked into all my antecedent experience, and into the experience of other men for whom I have the greatest esteem as investigators, it was more easy for me to believe the error of my manipulation, to believe that I had adopted defective modes of experiment, than to believe that all this antecedent experience was untrue. It was my own work that was thus brought to the bar of judgment, and my conclusion was, that I was far more likely to be in error than that the great amount of evidence already brought to bear upon the subject should be invalid and futile. Hence, instead of jumping to the conclusion that these were cases of spontaneous generation, I simply redoubled my efforts to exclude every possible cause of external contamination. This was done by means of doing away with the pipette altogether and using what we call a separation-funnel. Here you have a chamber with a pipette entering. This pipette-tube has not a bulb or mouth such as you have here; it is simply closed by a tube of India-rubber, and that again is closed by a pinchcock. Now, here we have an infusion of hay. At present this stopcock stops it. I turn it on; it goes down; I turn it off, and this liquid column is now held by atmospheric pressure. This was introduced into the India-rubber tube, the India-rubber tube being first filled with the infusion, so that no bubble of air could get in. When the separation-funnel was placed thus and the cock was turned on, the liquid was introduced into the chamber without an associated air-bubble. Mr. Cotterell will show you the result of this severe experiment. Here is an infusion of cucumber, the most refractory of all infusions that I have dealt with. It was prepared on December 8, 1876, so that it is between six and seven weeks old. Two days were sufficient to break down this infusion when contamination attacked it; but, by this more severe experiment, it is enabled to maintain itself as clear as crystal, although it has been there for six or seven weeks. You will see by the light behind that it is, as I have described it, perfectly clear. You will observe that the infusion is diminished by evaporation, but it is as clear as distilled water, and there it remains as the result of this severe experiment.

Let us now ask how it is that these curious results that I have brought before you were possible; how it is that the results of this year differ so much from those obtained previously. The investigation of this point is worthy of your gravest attention. I am now

called back to the experiments with which the inquiry this year began. As already stated, it was begun in September, and, leaving out the earlier experiments, I passed on to October 30th. I have now to bring your attention back to the earlier experiments performed in the laboratory. They were suggested by the ingenious investigations of Dr. William Roberts, of Manchester, and by the subsequent investigations of a man to whom we are indebted more than to any other for the knowledge we possess of the different species of those small organisms that we call bacteria; I refer to Prof. Cohn, of Breslau. Let me say that I entertain the very highest opinion of the intelligence and ability with which Dr. Roberts has carried out these experiments; they are in the highest degree creditable to him. This is the experiment to which I refer: Some chopped hay is put into a little can; it is raised to a temperature of 100° to 120°; it is kept for three hours, then poured off and filtered. Last year, we found that hay thus treated was sterilized by five minutes' boiling. I mean that, when it is exposed to the air that has this floating matter removed from it, it never shows any sign of microscopic life. Now, if you examine this natural hay-infusion with litmus-paper, you will find that it turns the litmus-paper red, showing that it is an acid infusion. Dr. Roberts found that acid infusions could be easily sterilized, and his mode of proceeding will be evident from the figure that I have here drawn. He took a vessel with an open neck at the top (*A*, Fig. 2), and filled it two-thirds full with the infusion he wanted to operate upon; he then stuffed the neck with cotton-wool, and sealed it hermetically with a spirit-lamp above the plug of cotton-wool (*B*, Fig. 2); he then placed it in a vessel containing cold water, and he gradually raised the water to a state of ebullition and maintained the boiling temperature for any required time. In that way he avoided all commotion, all evaporation, all ebullition in the infusion. After he had placed the tube in this condition in the water, and subjected it to a boiling temperature for any required time, he took it out and simply filed across the neck and broke it off, as I do with this one (*C*, Fig. 2). Here you have the infusion practically exposed to the atmosphere. The plug intervenes to prevent the entrance of dust, and still allows an interchange between the air of the bulb and the air outside. When Dr. Roberts took this acid infusion and neutralized it by the addition of caustic potash, he found it to possess the most extraordinary power of resistance to heat; he found that, in some cases, it required more than two hours to reduce this infusion to sterility; he also found that, in a particular case, it actually required no less than three hours' boiling to produce this effect. This was very different indeed from the results that I had obtained last year. I made many experiments with hay-infusion, and in every case we sterilized it by five minutes' boiling. I was led to take up the subject this year through the emphatic manner in which Prof. Cohn corroborated the results of Dr. Roberts.

I operated sometimes with tubes like those of Dr. Roberts, and sometimes with those which I call Cohn's tubes. These are formed by heating a certain portion of a test-tube and drawing it out so as to leave an open funnel above, a bulb below, and a narrow tube between both. These are Cohn's tubes. His method was this: He placed the tubes, as they are placed here, in boiling water, and, when they had been subjected to a boiling temperature for a sufficient time, he

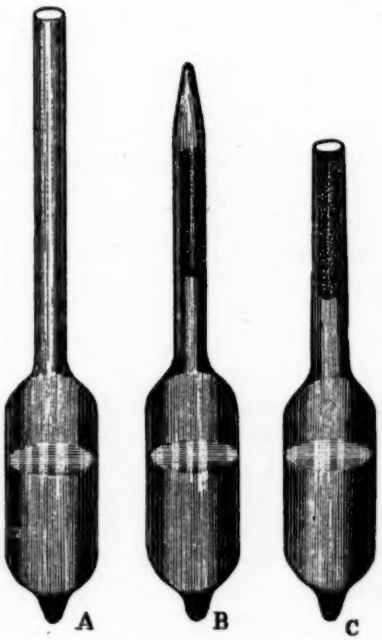

FIG. 2.

simply lifted them out. He found a certain amount of water condensed upon the neck of the bulb; he waited one or two minutes until that evaporated, and then quietly plugged his tube with cotton-wool, and he thought that this was perfect immunity against the entrance of contamination; and Prof. Cohn is very emphatic in saying that there is no thought of contamination from without in pursuing this method of experiment. I operated upon a great variety of hay-infusions, and after a time, by pursuing with the most scrupulous exactness the method laid down by Dr. Roberts and Prof. Cohn, it was possible for me, by practice, now to corroborate and now to contradict them. It is perfectly useless to bring forward before public assemblies merely opposing assertions, so that I did not really content myself with falling back upon the results I obtained last year, but tried to get some knowledge as to whence the differences arose which showed themselves between me and these distinguished men. Here

are tubes of alkalized hay, some of them subjected to a boiling temperature, not for three hours, but for ten minutes, and they are perfectly brilliant; there is not the slightest evidence of life in them; they have been entirely sterilized by an exposure to a boiling temperature for ten minutes. If I illuminate them, you will find that these infusions are perfectly brilliant; there is no turbidity that gives any sign of the production of animalcular life. These tubes have remained there for three months perfectly intact, uninvaded by those organisms which were invariably found both by Dr. Roberts and by Prof. Cohn. Again, we turn to another series of tubes, and find that every one of them has given way. Thus I went on ringing the changes, until, as I have said, it was in my power, by pursuing with undeviating fidelity the mode of experiment laid down by Dr. Roberts and Prof. Cohn, to get at one time a contradiction, and at another time a corroboration of their results.

And what was the meaning of these irreconcilable contradictions? The meaning was this: when we came to analyze these various infusions, we found that those that were sterilized by a boiling of from five to ten minutes were invariably infusions of hay mown in the year 1876, whereas the others were infusions of hay mown in 1875 or some previous year. The most refractory hay-infusion that I have ever found was in the case of some Colchester hay five years old. Now, what do these experiments point to? The answer may be in part gathered from an observation described in the volume of the *Comptes Rendus* for 1863, by one of the greatest supporters of the so-called doctrine of spontaneous generation. A description is here given of an experiment that was made by the wool-staplers of Elbœuf. They were accustomed to receive fleeces from Brazil, which were very dirty, and had, among other things, certain seeds entangled in them. These fleeces were boiled at Elbœuf sometimes for four hours; and the seeds were afterward sown by some of these expert fellows that had to deal with the fleeces, and were found capable of germination. The thing was taken up by Pouchet. He gathered these seeds, exposed them to the temperature of boiling water for four hours, and then examined them closely; and he found (and I recently made an experiment which showed the same thing to be true with regard to dried and undried peas) that the great majority of the seeds were swollen and disorganized, while the others were scarcely changed; they were so indurated, and perhaps altered in the surface, as to prevent the liquid from wetting them. At all events, a number of them appeared to be quite unchanged. He separated these two classes of seeds and sowed them side by side in the same kind of earth. The swollen seeds were all destroyed; there was no germination; but in the case of the others there was copious germination. Here, then, you have these seeds proved to be capable, by virtue of their dryness and induration, of resisting the temperature of boiling water for four hours. There is

not the slightest doubt that, if time permitted, I could heap up evidence of this fact, that the wonderful sterility of this old hay is due to the induration and desiccation of the germs associated with it. Here you have three tubes containing cucumber-infusion of crystalline clearness; they have been simply subjected to a boiling temperature for ten minutes; they have been completely sterilized, and they are as clear as when the infusions were first introduced into the tubes. On the other hand, here are tubes that have been subjected to a boiling temperature for five hours and a half, showing a swarming development of life. What is the reason of this difference? The reason depends entirely upon the method of experiment. When Dr. Roberts filled his bulbs, he simply poured in his infusion, plugged his tube, sealed it, and subjected it to a boiling temperature. Not only did the liquid contain germs, there was a quantity of air above the liquid, and the germs were diffused in the air. Germs thus diffused in the air are very differently circumstanced from germs diffused in a liquid;

FIG. 3.

they can withstand for hours a boiling temperature; whereas that self-same temperature, brought to bear upon germs immersed in liquid, destroys them in a few minutes. And why do these tubes differ? The reason is to be sought entirely in the method of filling the tubes containing the clear infusions. Here is a diagram (Fig. 3), representing one of Dr. Roberts's bulbs. You see that the top is united to a T-piece with a collar of India-rubber. This comes down and ends in

the neck of the bulb. Here is an air-pump, and here is the end of the T-piece surrounded by a tube of India-rubber, and here is a pinchcock to close that tube of India-rubber. If you open the pinchcock and work the air-pump with which this end is connected, it is completely exhausted. You may allow it to be filled with air; you may then open the pinchcock: the air will enter through the cotton-wool, and will fill the bulb. In this way you get the bulb filled, not with common air, but with filtered air. This process is carried on three or four times, so as to make sure that the common air has been displaced by the filtered air. We will suppose that I detach the tube from the air-pump, and other precautions taken. At present, you see the bulb is empty. Taking an infusion of hay, I put the end of the T-piece into the infusion to be introduced into the bulb. The bulb is dipped into hot water; the air expands, and it is driven out. Simply introducing our bulb into cold water, the air shrinks, and by atmospheric pressure the liquid is driven into the bulb. Again we drive the air out, and, by a few operations of this kind, we find that we can charge our bulb with a very great degree of accuracy. You can see the liquid in the bulb at the present time. In this way we charge a bulb which has had its common air and floating matter removed with our infusion. When it is charged, it is very carefully removed, and great precautions are taken so as to prevent any indraught of air. For instance, it is always removed from the cold water, so that, when it is lifted up into the air of the laboratory, a slight expansion shall take place, so that the motion of the air shall be from within outward, instead of from without inward. In that way we can, by careful manipulation, obtain bulbs devoid of this floating matter. These are the bulbs you now see before you showing this beautifully pellucid infusion.

Were this a biological investigation, and not a physical one, I should feel myself out of my element in dealing with it. I leave the determination of the species of bacteria to others far more competent than I am. I can see these organisms and wonder at them when I see them through the microscope; but I have no ability or knowledge to classify them and divide them into species, genera, etc. But these are purely physical experiments, and it is only by such severe experiments that this question can be freed from the haze and confusion in which it has been hitherto involved. Even the celebrated Prof. Cohn —I say it with the greatest regard and respect for him—appears to have no adequate notion of the care necessary to be taken in experiments of this kind. To lift a tube out of the boiling liquid and allow it to remain quietly in the air, the entry of the air taking place from without inward, and then, after one or two minutes' exposure, to plug it with cotton-wool, and say that no contamination can reach it, is in my opinion a great mistake. He could not, but by the merest accident, get an infusion free from contamination by operating in this way. I have here tubes prepared according to this method. Here

are some melon-tubes all putrid, all gone into a state of fermentation. I ask you to compare those with some other melon-tubes that I have operated upon in a different way and that are as clear as crystal. The others are all gone, simply through a defect in the mode of manipulation.

The defeats that I at first described to you were due entirely to the contaminated atmosphere in which we worked. It ought to be noted that, in the earlier experiments in this inquiry, the results were always in accordance with those brought before you last year. By degrees, however, masses of hay were introduced into the laboratory —old hay and new hay from various places; and they ended by rendering the atmosphere so virulently infective that everything was contaminated by the germs set afloat. It resembled the case of a surgical ward of a hospital, where gangrene and putrefaction have attained such a predominance that the surgeon has, in despair, to shut up his ward and abandon it to disinfection. Desiring to free myself from this pestilential atmosphere, I wrote to my friend the President of the Royal Society, Dr. Hooker, and I found that he was able to furnish me with a means of getting away from it. In Kew Gardens, there is a beautiful new laboratory, erected by the munificence of that most intelligent supporter of science, Mr. Thomas Phillips Jodrell. He, at his own expense, has had this beautiful laboratory built—being designed, I believe, by Dr. Thiselton Dyer. It is one of the neatest things I have ever seen, and it is to me a great gratification that the first experiments made in that laboratory were those to which I have now to refer. I broke away from the contaminated air of the Royal Institution. It is very well for you that I can tell you that all the germs referred to are perfectly innocuous to human beings, for I have no doubt the air of this room is contaminated with them. A series of chambers was made—not of wood, for I wanted to get rid even of that, but of tin—and I would not allow Mr. Cotterell to carry those chambers into the Royal Institution at all. They were carried from the tinman's where they were made to the laboratory at Kew. There, with the greatest care, the tubes were treated first with carbolic acid and then washed with water, and then with caustic potash to get rid of all traces of carbolic acid, and finally drenched with distilled water. Carbolic acid, as you know, is a deadly foe to these germs. In this way I hoped that every contamination that might be adhering to the tubes would be destroyed, and that, having got clear of an infected atmosphere, we might get the same results as we invariably obtained last year. The temperature was raised to between 80° and 90°, and once a little above 90°, so that the warmth was all that could be desired for the development of those organisms. It gives me the deepest gratification to find that what was foreseen has occurred, and that this very day these chambers have come back from Kew perfectly intact. They comprise the most refractory substances that I had experimented

upon here. It was almost impossible to save a cucumber; I never did succeed in saving a melon infusion from contamination, and from this so-called spontaneous generation. But here, when the air had been allowed to deposit all its motes, and when we were withdrawn from an infected atmosphere, as I have said, the chambers were returned with their infusions as clear as crystal. Mr. Cotterell will show you some of them. You will see that one of these is muddy and turbid, and it has a deposit at the bottom. These are all dead bacteria, and the muddiness is due to swarming bacterial life. Here you have two infusions perfectly clear. Why does the other tube give way? When we came to examine it, a little pinhole was found at the bottom of the chamber, and through that pinhole the germs got in. Here is a melon-infusion; and, in order to show you what would have occurred if the infusions had not been protected from the floating dust of the atmosphere, we have hung beside this case two tubes that have been exposed to the common air and have fallen into a state of utter rottenness. In this way, from the Jodrell Laboratory at Kew, we have had these cases returned with their infusions perfectly intact. Even in our infected atmosphere, when we subject our infusions to experimental conditions sufficiently stringent, we are able entirely to shut out contamination, and to show that spontaneous generation never occurs. When we get clear of our atmosphere altogether, this is a matter of perfect ease; and we find in Kew Gardens that Nature runs her normal course.

RELATIONS OF THE AIR TO OUR CLOTHING.[1]

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THE committee of the Albert Society has honored me by an invitation to give a few popular lectures at Dresden, on subjects of public hygiene. Let me state to you at once what I think of popular lectures in general.

What ought they to be, and what can we expect from them? I am not one of those who, in all their work and aim, look out directly for the practical use, for the return on the capital, immediate or prospective; but, on the other hand, I feel myself bound, in a certain degree, to inquire into the object of much that may appear to be either unprofitable or useless.

There is no doubt that *popular* lectures on *scientific* subjects will not impart really competent knowledge, and will not form experts.

[1] Abridged and translated by Augustus Hess, M. D., member of the Royal College of Physicians, London, etc.

Therefore it will be maintained by many that such lectures produce more evil than good, creating as they do, and augmenting, that dilettanteism from which our period is already suffering. In our schools also this dilettanteism is gaining such dimensions, that one might get thoroughly frightened at the immoderate expansion of young people's knowledge, were it not for its small depth, which lessens the danger, and for the fact that the forgetting keeps pace with the learning. Accept my open avowal, that I also am unable to invalidate the objection that *popular* lectures on *scientific* subjects are not able to impart a really competent knowledge, and do not form experts.

But I believe that this does not matter, and that they have no such purpose. They are neither an exhaustive, scientific, nor a practical instruction, but a scientific edification and elevation, which are to raise our minds and hearts, and to affect us like listening to good music—to a symphony, the purpose of which is certainly not to make musicians of all the listeners. It is sufficient to feel the harmony which lies in the nature of good music. There is harmony in all our knowing and doing, our aiming and striving, as far as there is *truth* in them, and fortunately the sense for perceiving this harmony is as widely spread among mankind as the sense for music. This harmony, which pervades every truth, ought to be brought home to the consciousness and feelings of everybody, so that the greatest number may rejoice and become interested in it, that we may approach new subjects, and perhaps make them our study, or that, at all events, knowledge and resulting sympathy may induce us to lend our help to those men whose profession and calling require them to enter more minutely and exactly into the subjects in question. In this respect popular lectures have a high and serious mission. It is their mission to create correct general ideas, to facilitate our grasp of them, to awaken and spread a certain love for different tasks of mankind and of the period, to form ties of friendship between things, ideas, and men. Sympathy and sacrifices cannot be expected or asked from us, if their objects are unknown to or badly understood by us.

For these reasons it is my desire to awaken your interest for some subjects relating to hygiene, and particularly to impress upon you most vividly how much in this respect remains to be done and created, a work we all ought to take our share in.

One of the incessant wants of man is air.

We want air mainly to nourish us and to keep us cool. The quantity of air inhaled and exhaled by an adult in twenty-four hours amounts on an average to about 360 cubic feet, or 2,000 gallons. What we take in and give out during twenty-four hours, in the shape of solid and liquid food, occupies on an average the space of $5\frac{1}{2}$ pints, which is equal to $\frac{1}{3000}$ of the volume of the air passing through our lungs. It will astonish you to hear, perhaps, for the first time that this amounts to 730,000 gallons in one year, and to be reminded of

that continuous work, which goes on day and night—a never-ceasing bellows-blowing, by which the organ of our life is kept in play. Of course, the quantity of air flowing round the surface of the human body is much greater than that. Do not object, that air is something so light that it need not be taken into account. It has some weight; water, certainly, is 770 times heavier, but our daily 2,000 gallons have for all that a weight of 25 pounds avoirdupois. Still, as it is not my intention to dwell here upon the subject of our oxygen-alimentation, I will to-day consider only the second use we make of the air, the cooling of our working machine.

You all know that life is bound up with chemical processes, kept in continual activity through the ingestion of solid and liquid food, and of oxygen from the air. One of the conditions for the normal performance of these processes is a definite temperature, above and below which they (although not brought to a standstill) go on differently—they leave off performing the functions of normal life—they lead to disease or death. With man this uniform temperature of his organs is one of the most essential conditions of his life. The blood of the negro living in the torrid zone of the equator is not by one-fifth degree warmer than that of the Esquimaux in the highest north at the coldest time of the year—it is always 99½° Fahr. The extremes of temperature under which human life exists are 95° to 104° Fahr. in the tropics, and 57° to 84° under freezing-point in the polar regions. There are even differences of 72° in the mean monthly temperatures of some countries, and yet the organs of man are everywhere of the same temperature.

By what means is man enabled to meet such colossal differences? What are his weapons for sustaining this gigantic struggle?

Let us look a little nearer into the absolute quantities of heat the living organism has to manage. The chemical processes going on in an adult person, within the space of twenty-four hours, produce about 12,000 caloric units. By caloric unit natural philosophy designates that quantity of heat which is necessary to raise the temperature of one pound avoirdupois of water by one degree of Fahrenheit.[1] By the heat produced by one person during one day about 660 gallons of water could be made warmer by nearly two degrees, or 7⅓ gallons could be heated from freezing to boiling point, from 32° to 212° Fahr.

Under certain conditions man produces more or less heat; for instance, according to the quantity of food he takes, or the degree of muscular exertion he undergoes, such deviations from the mean amounting at times to 50 per cent. of the whole quantity; but it is always the task of the body, and a strict condition for the maintenance of health, to keep the heat of the blood substantially the same, or at least within two degrees.

We have to look upon ourselves as warm and humid bodies placed

[1] Rankine's caloric units are used by the translator.

within a cooler atmosphere. Such bodies lose their heat in three different ways: 1. Radiation. 2. Evaporation. 3. Conduction. This triple arrangement is of great advantage for the heat-department of our organism, inasmuch as the existence of these different routes allows of a delicate regulation—that, for instance, which we lose in a given case by radiation can be made up by diminution of loss through the other routes, and *vice versa*. The losses by radiation and by conduction are the most constant under equal conditions, and evaporation of water is the principal means for equalizing differences resulting from varying production of heat or from difficulties of the two other routes. Allow me to illustrate this by drawing your attention to some every-day phenomena.

You arrive, for instance, in an hotel after a journey during a cold winter's day, and have at once a fire lit in your room. Let the fire be ever so bright, the thermometer even rise to a reassuring degree—you must stick to the fireplace; the room does not get warm. If you continue to live in the same room and have the fire kept in, it will by-and-by get comfortable even if the thermometer in the room should stand lower than on the first day, and you will think quite correctly that the room wanted time to get warmed through and through. Before that had taken place, the loss of heat by increased radiation into the incompletely warmed space made itself sensibly felt in the heating department of your body. Radiation is the stronger the greater the difference of temperature between the two bodies. Surrounded as you are in a room not only by air, say of 68° Fahr., but also by walls, furniture, etc., which stand, perhaps, at 38° to 40°, your body radiates its heat particularly toward these colder objects, till they also get warmer. For a room to be warm, it must get warmed with all which it contains.

Let us now look at the contrary case, when our loss by radiation is uncommonly limited; for instance, in a thronged room on a warm and moist day. You feel an oppressive heat, and scarcely trust the thermometer, which marks only 68°, perhaps your favorite temperature. Quite correctly, you accuse the throng of people, and retire into an adjoining room, where you find the air delicious, and seem to receive new life; there, again, the thermometer is suspected by you, as it is scarcely different from its colleague inside; and if the air in the two rooms were to be examined eudiometrically, the difference would be so small as to leave unexplained the difference in your sensations. What, then, causes this difference? It is the suppression of your lateral radiation of heat, when you are in the midst of other equally warm bodies; your receipts and expenditure by radiation cover each other, and the cooling of the individual limits itself to the two other routes, conduction by the air moving round him, and evaporation of water from his surface. On such occasions the pores of your skin pour forth a quantity of water, and, at the same time, you

instinctively try to increase the movement of the air—that means, its quantity in proportion to your surface; you want to increase your loss by conduction, and, if possible, by evaporation, and take to fanning, in order to facilitate the departure of your rising heat by the two open routes.

The loss by radiation can be very considerable under certain circumstances; 50 per cent. of the whole quantity of heat generally going that way, it is obvious that radiation deserves our full consideration. Particularly, an unequal radiation may be very injurious, such as takes place when a person is sitting or lying near a cold wall which is not covered by some bad heat-conductor, or near a window, etc.

On school-forms, the exposed sides of the first and last pupils are always more cooled than the sides directed toward their neighbors. In this respect there are numbers of practical points which are far from being sufficiently taken into consideration.

Let us now consider some instances in which the abstraction of heat by evaporation is predominant, or preëminently felt. The best known is that experiment by which one tries to learn the direction of the wind when the air appears calm and the sky cloudless. The moistened forefinger feels colder on that side which looks toward the wind, because more evaporation takes place there. The experiment does not succeed so well when the air is moist, because the moisture in the air prevents further reception of moisture by it; in our case, preventing the evaporation from the moistened finger.

Our organism acts similarly in all cases where there is an increased production of heat in our body, or where less heat is sent away by the two other routes. It has the power of dilating or narrowing the small blood-vessels in our skin and internal organs. The blood-vessel nerves which govern this motion are not subject to our will, but liable to be excited by external causes. When a person blushes, he gives off heat, because more blood rushes into the dilated blood-vessels of his cheeks and periphery generally, and more heat leaves the body. Under similar circumstances the whole surface of our body becomes fuller of blood and warmer, there is more heat to radiate and to be conducted away, and to be consumed by increased evaporation of the watery part of the blood.

The great value of evaporation for the cooling of our body can be estimated by calculating that as little as fifteen drops of water requires 2¼ caloric units to be changed into vapor.

We have at Munich a great apparatus for studying the process of respiration. It was given by the late King of Bavaria, Maximilian II., to the hygienic department of the university. Prof. Voit and myself have, by aid of this apparatus, investigated the quantity of water evaporated by men and animals during twenty-four hours. The constant result was that, under other similar circumstances, the quantity of evaporated water always rose in proportion to an increased meta-

morphosis of tissue, whether this increase was the consequence of increased nutrition, or of muscular exertion. We have experimented upon men at rest and at work, and we have found that on a day of rest they usually evaporated through lungs and skin about two pounds only during twenty-four hours, and on a day of hard work 4¼ pounds of water. In the first instance, about 2,016 caloric units, in the second, 4,480 had to leave the body in consequence of evaporation.

This explains to you how it can be that even with the hardest work our blood will not become warmer, but sometimes even cooler. The last observation has been made quite recently in mountaineering expeditions. Prof. Lortet, of Lyons, found, when he made an ascent of Mont Blanc, that the temperature in his mouth and armpit was less than normal, and became normal only when he was at rest. On such high mountains the lessened pressure of the atmosphere favors the peripheric circulation, there is a rush of water to the surface, and its evaporation takes place more readily, and increases with the altitude. At great heights persons in a balloon constantly complain of great dryness in the mouth.

Profs. Voit, Recknagel, and myself, are just now occupied in investigating the economy of animal heat, and we have found that after six hours' hard work the person leaves the apparatus in a cooler condition than when he went in, or after he had been at rest in the apparatus for the same space of time. Of course, the ventilation of the apparatus must work well, and send per hour about 11,100 gallons or 1,800 cubic feet of air through the chamber, else less water and less heat depart by evaporation.

You see what powerful means of cooling our body we have in the increase of our peripheric circulation, and consequent evaporation, at a time when the other routes are not open sufficiently—but you see also how dangerous this means can become, if it is employed at a time when considerable quantities of heat depart on the other routes. If, heated and damp, you enter suddenly a cold space, where radiation increases at once, and a good deal of heat is also yielded by conduction to the cold air, you are in great danger of contracting an illness by the abnormal losses of heat, and the violent and sudden changes in the circulation. But if you undergo such changes slowly and gently, the three routes open themselves harmoniously. Our organism is a faithful and clever servant, who helps himself and his master, provided he is not hurried and ill-treated. When I come to speak of ventilation, I shall not forget to tell you of currents of air, called draughts.

The third route, that of conduction, by which we give up heat to the air, is also of great importance, and must in some circumstances replace the two others to a considerable degree. As long as our body is warmer than the surrounding air, this air gets warmer at every point of contact with our body, but at the same time lighter, and as

such it is displaced by colder and heavier air, which in its turn gets warmer and lighter, and so on.

Each person standing in the still air of a room causes in this way an ascending current of air, just like a heated stove. A very sensitive anemometer, placed between coat and waistcoat, shows the existence of this current, which is strong enough to set the little wings of the instrument in play. The air in this room appears quite still, and yet it is in thousand-fold motion and ceaseless restlessness; but, happily, our nerves are not aware of this, just as a short-sighted person may deny the existence of some object, till his eyes get the assistance of a glass. Whoever of you would be able to feel or see all the movements of the air in this room would probably not be able to stand it. A correct idea may be formed about it by the action of smelling substances. If, for instance, an escape of gas were to take place in a remote corner of this large room, you would become aware of it almost immediately all over the room. Our nerves are happily so organized that they begin to feel the motion of the air only when it amounts to about 3¼ feet per second.

You may have some doubt about this ignorance of your nerves, because the proof lies not in our immediate perception, but in conclusions from other observations; but you may easily convince yourself that it is so. It is the same thing whether you move your hand at a certain rate through a still air, or whether air moves at the same rate round your hand. You will find that you do not feel anything, no resistance, no coolness, if you move your hand at less than 19 inches per second.

I take this opportunity to draw your attention at once to the average movement of air out-of-doors, a subject very imperfectly known to most people, but which you must understand well in order to have a correct idea of the real difference between being in a room and in the open air. The velocity of the air is measured by an instrument called an anemometer, a description of which you can easily get at. In our temperate climate this velocity amounts on an average to about 10 feet per second. This would make about 7 miles per hour. Imagine a frame about the height and width of a human body; let us say it measures about 6 feet by 1½, or 9 square feet. If you multiply this by the average velocity of the air, you will find that in one second 90 cubic feet, in one minute 5,400 cubic feet, in one hour 324,000 cubic feet of air flow over one person in the open. I shall come back again to these numbers when we have to consider the subject of the ventilation of dwellings, but you will already understand that it is not too much if 2,100 cubic feet of new air per hour and per bed are considered necessary in the ventilating arrangements of hospitals, etc. This quantity, which appears large, is after all only about $\frac{1}{150}$ of the quantity of air which comes in contact with a person in the open at the above stated average velocity of the air.

You see, therefore, that we give off more heat by conduction in the open air than in a room, and in the latter proportionately more by radiation and evaporation.

The power of conduction is best appreciated when we change the air for some other fluid medium, which is a better conductor than air, and more capable of absorbing heat, I mean water. In air of a few degrees of heat, we can feel pretty comfortable with moderately warm clothes; but, if with the same amount of clothing we were to get into water of the same temperature, we should feel painfully cold, and should probably be frozen to death in a few hours, although our loss by evaporation would have ceased entirely, and that by radiation nearly so. In hot climates, therefore, a daily bath is of great service for the necessary cooling of our body, even if the water is not cooler than the atmosphere.

In the air also the loss of heat by conduction is the greater the lower the temperature, and the greater the velocity of the air which flows around us. This explains on the one side why it appears superfluous in a calm and cool air to make use of a fan, while this expedient acts so beneficially at higher temperatures; and on the other why, as a rule, a warm air in motion appears much cooler than a calm one of equal temperature. Think of the sultriness before a thunder-storm, as long as the air is at rest, and how differently we feel as soon as the first wind rises. The air is not yet cooler, not less saturated with vapor than before, and still it deprives us of so much more heat that we deem it less sultry, even cool, only because it travels over us faster.

When we fan ourselves in a hot and damp air, the same thing takes place—then, also, a greater amount of air passes over us in a given time than if we leave the air to its own motions. The fan changes nothing in the temperature and moistness of the air, it only increases its velocity, and in consequence the abstraction of heat, and thus affords us coolness chiefly on the uncovered or only slightly covered parts of our bodies; therefore, ladies have more reason for using it than the stronger sex.

As long as the air is our surrounding medium, an increased evaporation associates itself with the increased loss by conduction, at least as long as the circulation of the blood in the skin remains active and the air is not saturated with moisture. The fan scarcely ever cools by increased conduction alone, but also by increased evaporation. Therefore, fanning with dry air is much more cooling than fanning with a moist air of equal temperature. We all know how much quicker wet roads and wet clothes dry when there is a good wind. However rapid the motion of moist air may be, it does not dry. When our body is bathed in perspiration the fuller condition of the skin occasions an increased transfer of heat from the dilated blood-vessels to the surrounding air by conduction, but generally also by evaporation.

In southern climes, at the hottest and moist time of the year, when

the body cannot lose much heat by radiation toward colder objects, when the temperature of the air approaches and even surpasses at times that of our blood, the European often feels the heat to suffocation, and besides the use of the bath he has no other practical remedy than the fan and the shade.

In the shade the air is not only cooler, but also more in motion. The difference of temperature between a place sheltered from the rays of the sun and a neighboring one exposed to them, produces a motion, a current, because bodies of air of unequal temperature are also of unequal weight. They are not in equilibrium, and seek to reëstablish it by motion. Any one may easily convince himself thereof who, on a hot day with calm air, walks alternately over places exposed to the sun and sheltered from it. As soon as he comes into the shade of a cloud, a house, or a tree, he feels at once a soft wind rising. The shade not only protects us against the direct solar rays, but it increases also the ventilation of the shady place.

The fan acts on the same principle. The *pankha* in the bungalow, by increased conduction and evaporation, keeps the blood of the European at its normal temperature of 99½°. When the temperature of the air rises to 140°, when the walls of the house or bungalow are no longer cool enough to provoke radiation from the heated human body, man is reduced to cooling by evaporation. It greatly depends upon the state of dryness of the air how far he succeeds. The drier the hot air is, the better is it able to withdraw water from the skin, from the respiratory organs, from the wetted floors, and consequently the more heat from the human body. The moister it is the less it is able to act thus.

In order to give you an idea of the quantitative differences in play, we will consider the losses of heat by respiration as they take place at different temperatures and different conditions of moisture of the air we draw in. In twenty-four hours the quantity of this air is on an average 2,000 gallons. It has been calculated that by the process of respiration a person loses 1,172 caloric units when the air is at 32° and quite dry, 1,116 when it is half saturated by water, 1,060 when it is completely so. The difference between the two extremes is only a small percentage of the whole loss. But, when the temperature is 86°, the above numbers would be respectively 1,096, 760, and 420.

A comparison of the losses of heat by the respiration of an absolutely dry and an absolutely saturated air at 32° and 86° Fahr. is highly instructive. We lose:

at 32° and dry	1,172 caloric units.
" 86° " "	1,096
	difference only 76
at 32° and saturated	1,060
" 86° " "	420
	difference as much as 640 caloric units.

The different state of dryness of the air appears thus to be of a greater moment than the difference of temperature, and this is the reason why our sensations do not always coincide with the thermometer. You readily understand how much more difficult it is to manage one's heat-household in a hot than in a cold climate. Our means for warming ourselves are better than those for carrying off our heat. Therefore the European race has a hard fight under the equator. The working power of the body depends on a certain amount of consumption, by which a certain amount of heat is necessarily created, which has to leave the body in a regular way. The Hindoo who has to draw the European's *pankha*, bears the heat better in proportion as he takes less food and creates less heat in himself, but then his working-power is also quite proportionate to the total of his consumption.

The European's struggle in a hot climate and his dangers of degeneracy will remain the same as long as he has no better means of cooling himself by some or all of the known three routes. Houses with thick stone-walls are tolerably efficacious. These walls rarely get warmer than the average temperature of the year. They cool the air which comes into the house, and act on the inmates in the way we have seen when speaking of the room which is not warmed through. A good means would be some contrivance by which the air in the house could be deprived of its water.

I could not help inflicting upon you this rather long introduction, nor could I possibly abbreviate it, as, without the little knowledge which I have tried to impart to you about the cooling of the human body, you would not be enabled to obtain a proper insight into the functions of our clothing and our dwellings. Therefore I believe myself to have had a good claim on your patience and indulgence.

One of man's principal defensive weapons in his struggle for existence is his clothing. The place it takes in the history of civilization and its connection with physiology are not often thought of. People speak about it generally from a moral and æsthetic point of view, but the main purpose of clothing is seldom approached in conversation—I mean the purely hygienic one. I deem this to be a misfortune, because this forgetting of the chief point has subjected mankind to the rule of small and frivolous considerations, and the manners and fashions of the period get frequently the better of the hygienic fitness of the clothing. Morality and beauty do not depend on dress. They cannot be created or preserved by it. These great qualities could even exist without it, but the human body as it is could not, or only barely and imperfectly, exist in our climate without the protection of clothing, which is more indispensable for our health than for our beauty and morality.

So manifold are the changes brought about in our system by clothing ourselves, that I am unable to give you more than some incomplete parts of the subject.

When I cover one part of my body I change the degree of abstraction of heat by all three routes known to you, but without obstructing any one of them entirely.

To speak in the first instance of radiation, it will be clear to you that our surface is prevented from radiating heat directly toward the colder objects in our neighborhood, and that it can only radiate toward the covering materials, which receive this heat. By the laws of conduction and radiation the heat, which has radiated from the body into the clothes, has to travel through them by radiation and conduction, till, arrived at their outer surface, it can radiate thence toward colder objects, just as it would from the naked surface of the body. Thus by our clothes we keep the heat radiating from us somewhat longer in the immediate neighborhood of our surface. The lightest covering even makes itself perceptible by impeding radiation, the thinnest veil keeps warm in some degree. It is just the same with the earth itself. On a calm, clear night the earth's surface becomes so chilled by radiation into the colder space, that the moisture of the air precipitates itself on it as dew, and at times as hoar-frost, and even as ice, just as the moisture in a warm room does on a window-pane cooled from the outside; but, when a veil of clouds overhangs the earth during the night, the earth never cools itself so much as to allow of any dew forming.

There are substances, called diathermal, which allow the rays of heat to go straight through them without any absorption, for instance, the crystals of common salt, but all the materials of our clothes are such as absorb the rays of heat which come to them from one side, and only part with them after they have reached the outer surface. The transit of heat through what we may call our artificial surface depends essentially on the conductive power of the material and its thickness, i. e., on the length of time and way which the heat has to go through in order to travel from our surface to the outer surface of the garment.

Thus the whole immediate neighborhood of our body is continually warmed in an even degree by our radiating heat, and our sensitive skin is spared the numerous disagreeable or injurious effects of a rapidly-changing temperature.

The heat does not remain in our clothes, it is continually on an outward move, faster or slower, and, to a certain degree, also warms the stratum of air between our clothing and our skin, so rich in nerves and blood-vessels. This air, as we shall see presently, continually changes, and must change if we are to feel comfortable. In the cold of winter, and in the open air, we lose our bodily heat out of our well-selected garments without any sensation of cold, only because we have removed the place of exchange between the temperature of our warm blood and the cold winter air from our sensitive surface to a substance without life and sensation; instead of our skin, our dress

feels the cold. It is the same with the hair of animals, and the feathers of birds, they are also without nerves.

In proportion as our heat-losses increase, while the creation of heat in our interior remains about the same, we feel the necessity of diminishing the rate at which the heat leaves our immediate neighborhood. This kind of regulation is somewhat taken care of involuntarily even by the naked body. In consequence of the cold, the nerves which act on the calibre of the blood-vessels of our surface contract them, and lessen the quantity of blood in them, so that less heat comes to the surface, and we need not be afraid of becoming also inwardly colder if we feel cold, even very cold.

The sensation of cold on the skin does not necessarily give the measure of our internal temperature. In the cold stage of ague, for instance, the temperature of the internal organs rises considerably, while by a kind of spasmodic contraction of the superficial blood-vessels the flow of heat toward the skin is less than normal. The above-mentioned regulation of heat-loss by the capillary system of our skin is not all-sufficient either in point of time or degree. The cold may be too strong, and the regulator get overworked and paralyzed, so that additional clothing is required to delay the departure of our heat, and to spare the nerves of the blood-vessels. We help ourselves by additional clothing, and the underlying article of clothing stands in the same relation to the outer one as the skin to its first covering. From this point of view you have to consider the sequence of shirt, under-clothing, coat, overcoat, etc., etc., an arrangement by which we save the vasomotor nerves the greater part of their work.

It is an open question, which the incompleteness of our hygienic knowledge prevents us from answering quite satisfactorily, how far we ought to hand over the regulation of our heat-loss to our dress, or how far we should go in deputing it to our organism, and its capability of transferring more or less heat from the centres to the surface of our bodies. This self-help of the organism and the readiness for it resulting from frequent exercise of this function are generally called hardening one's self; the contrary, making one's self tender. The former we can never quite dispense with, but I believe that too high a value is sometimes put and too large claims made on it. One ought to possess the capability and the readiness, but not to make use of them continually.

All human aim must be to obtain the greatest effect at the smallest expense. We ought to choose those means which attain the end without exhausting our power, which should be preserved for higher purposes. These principles ought to guide us in approaching the question. It is not only superfluous, but positively injurious, to use one's self up.

I believe that it is now evident to you that a part of the heat of our body radiates from the surface of our clothing; but we must now consider whether this radiation does not vary according to the nature, quality, or color, of the material. Experiments which have been

made by Dr. Krieger on wool, wash-leather, silk, cotton, linen, and India-rubber, have not shown any important difference. Krieger covered cylinders made of tin and filled with warm water with different and differently-arranged materials, and noted the decrease of temperature in stated periods. He used layers of two different materials, but it made no great difference what the outer layer was. Still, I will mention that silk and cotton allowed more heat to radiate than wool. The color also of the material has been shown to have no great influence on the radiation of heat, which remains the same, whether we have a black or a white garment on.

But it is quite another case when we receive luminous heat, rays of heat proceeding from luminous bodies, such as the sun, or some flame; then differences result, which certainly are not very great with different materials of the same color, but become great indeed when the colors are different. For white textures the following proportions are found:

When cotton received	100
Linen received	98
Flannel "	102
Silk "	108

With shirtings of different colors the proportions were:

White	100
Pale straw-color	102
Dark yellow	140
Light green	155
Dark green	168
Turkish red	165
Light blue	198
Black	208

Of course, you all know by experience that, when dressed in black, you feel much hotter in the sun than when dressed in white. It is remarkable that, pale straw excepted, each color heightens considerably the absorption of luminous heat-rays, and that blue does so nearly as much as black. But, as soon as we are in the shade, the differences nearly vanish.

If we continue to consider our loss of heat by radiation through and from our clothing (omitting for the present conduction and evaporation), we come at once to the practical question, how much this loss is retarded by interposing several strata of material between our surface and the air, or in fact to the question about the heat-conducting power of materials and textures. Very few experiments have been made in this respect. We know, with respect to this point, the properties of metals, of minerals, of chemical compounds, but not of wool, linen, or leather. This shows, by-the-by, how little hygiene has been treated until now in an exact and scientific way. We talk in a general way about the use of garments as bad conductors of heat,

but the few experiments known to me entirely run counter to our accepted ideas.

Krieger experimented on cylinders filled with warm water, by surrounding them tightly with single or double textures. As the loss by radiation is the same in both cases, any difference must result from difference of conductive power in the coverings of the cylinders; but the results were, for the most part, surprisingly small. The following numbers represent the proportions of loss of heat through double tight-fitting coverings in comparison to single ones; the losses through the single ones being taken as 100, they were, through—

Double thin silk	97
" Gutta-percha	96
" Shirtings	95
" Fine linen	95
" Stout silk	94
" Thick home-spun linen	91
" Chamois-leather	88–90
" Flannel	86
" Summer buckskin	88
" Winter buckskin	74–84
" Double stuffs	69–75

The whole question is certainly not exhausted by these experiments, but one thing becomes evident by them, that it is not the substance and its weight, but the texture and the volume, which are the principal causes of the difference. Thin and stout silks, fine and stout linen, are nearly equal in substance, and equal sizes of them are not so very different in weight; it is their different heat-conducting power which causes the difference of the loss, and this is, even through two layers of them, not as much as ten per cent. smaller than through a single one.

By other experiments one can demonstrate that, by changing the shape and volume of the same substance, great changes of heat-loss can be produced, although the substance and its weight remain the same. If you cover the tin cylinder, previously filled with warm water, with common wadding, and observe the falling of the immersed thermometer, you will be astonished to see how rapidly the fall goes on, as soon as you compress the wadding firmly and diminish its volume: the outward flow of the heat increases by forty per cent. The same, you know, is the case, when a wadded garment is worn out; the quantity of the wadding is the same, but its volume and its elasticity have undergone a change, and you will find it considerably less protecting.

This observation leads to another instructive experiment, relating to the influence of double layers of material. If the first layer only is tightly drawn over the warm cylinder, and a free space of one-third to one-half an inch between it and the second, which may be compared to a comfortably-fitting garment, the second layer very considerably lessens the outward flow of the heat. The amount due to conduction

being deducted, the impediment by the second layer is about the same for different materials, but very considerable for each of them:

For Linen	32 %
" Shirting	33 "
" Silk	32 "
" Flannel	29 "
" Wash-leather	30 "
" Gutta-percha sheeting	36 "

From this follows the practical truth, that we can produce a very different effect on our body by the same number of clothes, according to the tightness and looseness in the make. Just call to mind tight shoes and gloves in winter-time!

This fact leads to a series of other facts, which contain the explanation why wadding, as long as it is loose and elastic, keeps you warmer than when it is once flattened. This is the air contained within the clothes.

One generally considers clothing as an apparatus for keeping the air from us. This conception is utterly erroneous; quite the reverse, we can bear no garments which do not allow of a continual ventilation of our surface. Just those textures which are most permeable to the air keep us warmest. I have examined different materials for their permeability to air, which can be easily ascertained. One closes a series of perfectly equal glass tubes with different textures, and observes how much air passes through the clothing substances at the same pressure during the same time. Taking the quantity of air passing through flannel as 100—

Linen allowed		58
Silk	"	40
Buckskin	"	58
Kid	"	1
Chamois	"	51 parts of air

to pass through them.

If our clothing kept us warm in proportion to its power of excluding the air from our body, kid would keep us a hundred times, and chamois warmer by one-half than flannel, and so on, while every one knows by experience that it is quite the reverse.

If there are several layers of the same material, ventilation loses but very little at the second layer, because the velocity of the air in its passage through the first layer remains about the same on its further progress, the following layers being like a continuation of the preceding ones, as if they were tubes of the same calibre, retarding the original velocity of a fluid by the amount only of unavoidable friction.

Thus, a current of air travels incessantly through our clothing. Its force, as in ventilation generally, depends on the size of the openings, the difference between the outside and inside temperature, and

the velocity of the surrounding air. We need not be anxious to make our clothes prevent the access of air to our skin; they have only to regulate and moderate it to such a degree that our nerves may not feel the air as something in motion. This degree is far from immobility. When in the open air we believe it to be quite calm, there is still a velocity in it of at least one foot and a half per second, or about one mile per hour, as you heard before.

Our clothing not only renders the air still around us, but it also regulates its temperature by the heat which leaves our body; we heat our garments, and they continually heat the air passing through the meshes and pores of the texture. We may compare our clothing to a calorifer or stove, warmed by the heat emanating from our body's engine for the purpose of warming the air round our surface.

We do not feel the loss of heat which our clothing undergoes as we should if the air were to strike our surface without having been previously prepared by our dress; the differences of temperature balance themselves within the material we are clothed in, and of which the ends of our cutaneous nerves form no part. Inside our dress we carry the air of the South wherever we may be. Its temperature averages about 75° to 94° Fahr. We live in our dress like an unclothed tribe in a paradisian country, where the air is constantly calm and the temperature 75° to 94°. It will be easily understood now why rough, loose textures keep us so warm, while newly-carded cotton-wool does so more than when old and compressed; why tissues of fine fibres and threads make the best material. Fur, of which you know so well the properties, consists of hair and skin. Chemically speaking, there is not much difference between skin and hair. In fur the weight or body of the skin is much greater than that of the hair, and still it is essentially the light hair to which the fur owes its warming properties.

There are some interesting experiments on this point. Krieger observed the flow of heat after covering his cylinders with unshorn and shorn fur. Putting down the loss of heat through the entire fur as 100, he found that it rose to 190 when the same piece of fur was used shorn. A dried skin, you know, is always somewhat porous. When he altered this by giving it a coat of linseed-oil varnish, the loss of heat rose to 258; and, when he took a solution of gum-arabic instead, it rose even to 296.

It has been proved that the living organism, in parting with its heat by radiation and conduction, behaves just like a tin cylinder filled with warm water. It is a yet older observation that furred animals, such as dogs, rabbits, etc., cannot live when they are shorn and their skin varnished or oiled. One used to explain their death by the suppression of the evaporation from their skin, but it can be proved that even in a comfortably-warm room these animals literally freeze to death. Krieger sheared a rabbit, after having noted its temperature

and frequency of respiration; they were 102° and 100 per minute. He did not use any varnish, to avoid any possible suppression of evaporation from the skin, but enveloped the shorn animal in a wet cloth. The temperature of the room being at 66°, the animal lost so much heat that, after five hours, its interior temperature had fallen to 75°, and its respiration to 50 per minute.

A fur is so arranged that its fine hair, projecting into the air, intercepts all the heat, which flows from the surface by radiation and conduction, and distributes this heat through the air, which circulates between the single hair-cylinders; the finer the hair of the fur, the more of the outgoing heat is taken up by the air, which, however cold the temperature may be, reaches the nerves of the skin as a warmed air. Furred animals, in winter, when touched superficially, give a very cold sensation; it is only near the skin that their hair feels warm. In severe cold, certainly little of our animal heat comes as far as the points of the hair, from which it would radiate or be conducted into the air; the current of air in the fur cools the hair from its point toward its roots, and a severer cold penetrates only a little farther into the fur, without necessarily reaching the skin of the same. This takes place only when the temperature is uncommonly low, and the air in violent motion. Travelers in high latitudes all agree that extreme degrees of cold can be borne very well when the air is calm, but scarcely so when there is a brisk wind.

This tends to show that in very severe cold the outflow of heat, by the skin into the air contained in the fur or within the dress, takes place through one route only—that of conduction; when a fur is worn, no heat comes to the surface for radiation, as soon as the points of the hair have the temperature of the surrounding air. Evaporation also sinks to a minimum, because at 68° Fahr. under freezing-point all formation of aqueous vapor already ceases, and nearly all the heat in the fur and the dress is employed to heat the arriving air, whose velocity increases according to the difference of temperature. In a well-furred animal the changes of temperature in the surrounding air only change, if I may say so, the latitudes of the cold and warm zones in the fur; the place where the temperature of the body and the air equalize each other moves between the roots and points of the hair, and for this reason such a well-furred animal is not warmer in summer than in winter. Its blood keeps always at the same temperature, because in summer a great part of its heat leaves at the points only of the hair by radiation and conduction, while in winter the heat departs already near the roots of the hair.

Air-proof fabrics ought to have only a very limited use. In India-rubber or gutta-percha textures we feel highly uncomfortable when we have to undergo much exercise, or have to give off more heat than usual. They become inconvenient, not because they stop the change of air entirely—which they cannot do in fact, on account of the neces-

sary openings in them—but only because they limit the universal exchange of air in the underlying garments. For protection against the wet from without they are well suited, but they produce another wet on our skin by impeding evaporation. They may be used in wet weather, when accompanied with cold or wind, but never, though wet, when it is warm or calm.

Finally, I have to draw your attention to the relations which the materials of our clothes have to water, by which their functions are considerably altered. They are all hygroscopic; that means that they condense from the atmosphere a certain amount of water. This hygroscopic property, very different in different bodies, increases with the decrease in the temperature of the air, so that all of them condense more water at freezing-point than at higher temperatures. Partly, also, the relative moisture of the air is of some influence, so that at 68° the hygroscopic body absorbs more water from an air nearly saturated than from a less moist air. As yet we do not know much about our clothing materials in this respect. I have made some preliminary researches, and have found unexpectedly great differences.

I took two equal pieces of flannel and of linen, as representatives of the two most important fabrics made of vegetable and animal fibres, and dried them at 212°, a temperature at which they lose all their hygroscopic water. I put them into well-closed boxes of known weight, and noted the weight of the two together. They were then exposed to the air in places of different temperature, and from time to time put back into the tin boxes, and the weights taken again. By this method it was not difficult to ascertain the relative quantities of hygroscopic water which the flannel and the linen had absorbed. These quantities are tabulated below, as they resulted from different localities, temperatures, and lengths of time, the weight of the linen and flannel being 1,000 grammes each:

OBSERVATIONS	LOCALITY.	TEMPERATURE.	TIME.	HYGROSCOPIC WATER IN	
				Linen.	Flannel.
1	Cellar.	37.58° Fahr.	12 hours.	77	157
2	Lecture-room.	34.16	" "	74	143
3	Room.	64.25	" "	41	75
4	Laboratory.	53.96	" "	63	105
5	Cellar.	39.92	" "	111	175
6	Lecture-room.	40.1	4 hours.	93	160
7	"	40.1	3 "	91	148
8	"	41.9	15 "	85	146
9	Room.	69.8	10 minutes.	73	113
10	"	69.8	" "	52	96
11	"	70.7	" "	45	87
12	"	70.7	" "	43	82
13	"	68.9	15 "	42	78
14	"	68	" "	42	77
15	"	64.25	30 "	41	75
16	"	62.6	1 hour.	48	76
17	"	61.7	2 hours.	45	77
18	"	59.9	..	46	78

What most strikes one is the invariably greater hygroscopic power of wool than of linen; the maxima and minima of flannel and linen being respectively 175 and 111, 75 and 41.

Observations 5 to 8 show that linen changes the quantity of its hygroscopic water at a proportionately quicker rate than flannel. The two pieces were for twelve hours in the cellar, when linen absorbed 111, flannel 175; immediately after, for four hours, in a cold place, where linen lost 18 per 1,000 of its absolutely smaller amount of water, while the flannel lost only 15 per 1,000; but during the next three hours linen lost only 2, but flannel 12 per 1,000.

When (Obss. 9 to 15) the pieces had come from the cold lecture-room into a warmed room, linen again ceased giving off water at a much quicker rate than flannel.

The accelerated rate, only in an opposite direction, took place again (Obss. 15 to 18) when the temperature in the room sunk from 65° to 59°.

All bodies become more hygroscopic with a sinking temperature, but the absorption of water and increase of weight, as well as the contrary process, take place proportionately quicker with linen than with flannel.

The more the air in any material is displaced by water, the less it keeps us warm, the quicker it conducts the heat; hence the frequent injury resulting from wet clothes, and the striking discomfort produced by a damp cold. You all know how comfortable we can feel in a walk, when the air is cold and dry, and how differently we feel when it is damp, although not colder. Then our clothes also get much damper, and conduct more of our heat away.

This is not to be underrated. We have seen in the table that 1,000 parts of flannel took up in the cellar 157 parts of water. Take the weight of a whole woolen clothing as ten pounds, and you see that it may absorb one and a half pound of hygroscopic water, which requires about 1,680 caloric units from our body to be evaporated.

Linen and flannel bear the same relation toward water they are wetted with as toward their hygroscopic water. Linen is quickly wetted and soaked, wool more slowly, but linen cannot take up the same quantity. Spilled water has certainly taught you this many times, when you wanted to take it up. It is the same in evaporation, which is also much quicker from linen. Two equal pieces of linen and flannel, weighing each 1,000 grammes, put into water and wrung out till they no longer yield a drop of water, keep back respectively 740 and 913 per 1,000.

But a much greater difference exists in the intensity of evaporation from wet linen and from wet flannel, during equal periods, in a heated room.

OBSERVATIONS.	TEMPERATURE.	MINUTES.	WATER TO 1,000 GRAMMES OF	
			Linen.	Flannel.
1	70° Fahr.	..	740	913
2	68	15	521	701
3	68	30	380	603
4	67	30	229	457
5	66	30	99	309
6	66	30	55	194

It is easy to see from this table how much quicker linen works than wool in every direction.

During the first 75 minutes there evaporated from 1,000 parts of linen 511, from 1,000 parts of flannel 456 water; afterward the reverse took place: in the following 30 minutes 130 evaporated from linen, 148 from flannel, and in the last 30 minutes only 44 per 1,000 from linen, but 115 from flannel.

It is also evident how much more evenly the drying proceeds in wool: in the first 15 of the whole 135 minutes 219 evaporated from linen, in the last 15 minutes 28 per 1,000, while with wool it was respectively 212 and 97 per 1,000. I must not forget to mention that all these experiments were made with pieces of nearly equal size and shape.

It is self-evident that all textures lose their permeability to the air in proportion to their state of humidity, the water partly at least obstructing the pores. Coarser stuffs with larger pores will keep their permeability longer; if the pores are equal, the difference in the adhesion of the water to the substances will come into play. Linen, cotton, and silk are very different in this respect from sheep's-wool. The former become very quickly air-tight by wetting, the latter scarcely so, or only after a longer soaking. Soldiers can tell how damp and vaporous the air becomes under a wet tent, and how quickly the tent becomes airy when it begins to dry.

As the porosity of all fabrics depends chiefly on the elasticity of the fibres of their material, it must be of great importance how far that elasticity keeps under wet and dry. There, again, wool stands apart; its fibres do not lose much elasticity when they get wet: it is not so with other fibres. Wet linen and silk are just like Krieger's shorn fur, when it was coated with varnish or gum-arabic. The greater facility of catching cold in wet linen or silk than in wet wool is in exact proportion to the greater facility with which water expels the air contained in their fibres. Many of you may have learned a lesson from a wet linen or cotton and a wet woolen sock.

On the other hand, there is an advantage in these materials if we want to keep ourselves cool and dry. By means of them we part with heat and moisture from our surface much quicker, and hand them over to other layers for further removal.

To be quite methodical I ought now to treat of the different parts

of our clothing and of the fitness of different materials for special purposes. But, to say the truth, Science has not yet done much in this direction.

There is still one of our garments to be considered which generally is not regarded as such. I mean the bed—that piece of clothing in which we spend such a great part of our time. It is equally indispensable to the sick and to the healthy, and at all times it was considered as a sign of bitterest want if a man had no place to lay his head.

The bed is not only a place of rest, it is especially our sleeping-garment, and has often to make up for privations endured during the day and the day's work, and to give us strength for to-morrow. You know all the different substances and materials used for it. They are the same as our garments are made from. Like them, the bed must be airy and warm at the same time. We warm the bed by our body just as we warm our clothes, and the bed warms the air which is continually flowing through it from below upward. The regulating strata must be more powerful in their action than in our day-clothes, because during rest and sleep the metamorphosis of our tissues and resulting heat become less, and because in an horizontal position we lose more heat by an ascending current of air than in a vertical position, where the warm ascending current is in more complete and longer contact with our upright body.

The warmth of the bed sustains the circulation in our surface to a certain degree for the benefit of our internal organs at a time when our production of heat is at the lowest ebb. Hence the importance of the bed for our heat and blood economy. Several days without rest in a bed not only make us sensible of a deficiency in the recruiting of our strength, but very often produce quite noticeable perturbations in our bodily economy which the bed would have protected us from.

I wish, therefore, to impress upon you that your charitable exertions for the poor may become extended to the bed, that kind of garment which can make up to a great degree for other lamentable deficiencies, as in food, dwellings, clothing, toward which you are in the habit of directing your efforts.

I am quite aware that I have anything but exhausted the subject of the functions of our clothes, but still I believe that I have directed your attention to such essential points as to convince you of the importance which a scientific consideration of the subject possesses in the interest of the heat-economy of the human body.

As our health is so intimately connected with this economy, a better insight into the laws and proceedings of the same must in the end turn out profitable to health in general.

Thus we have learned in our last glorious war how important it is to provide well for the soldiers' clothing, and that a few days' want

of provisions is less injurious to the health of the soldier than perturbations of heat-economy through want of suitable pieces of clothing. Our clothes are weapons with which civilized man fights against the atmosphere as far as it is inimical, the means by which he subjugates this his element. It lies in our nature, in our instinct, in our self-respect, to have good clothes, which ought to be also pleasing to the eye; but we ought to become more conscious of their purpose. Ornament must be the minor consideration, and the tailor ought not to hold his scissors as a sceptre over the hygienic purposes of all dress.

Our period strives after novelty in all directions, also after new forms and styles in dress, architecture, and so on; but nothing new will be created with our old points of view remaining. New points of view can only be gained by new and increased insight into the functions of the dress and the house. This function must determine the form, and will not be ascertained without theoretical study. It was not till we had mastered the theory of the overshot and undershot water-wheel that the turbine could be invented.

The influence of theory on practical development is much greater than is usually supposed and conceded. The discovery and settlement of the laws of mechanics had to precede their application to engines, railways, steamboats, and so on. There would be no difficulty in showing why the great inventions of Watt and Stephenson were not made at an earlier period, and that they were the fruit of seeds which were buried in the theoretical investigations of Copernicus, Kepler, and Newton.

Perhaps our future means for keeping our heat-household will be as different in style and appearance from our present ones as a turbine from an old mill-wheel, or a steam-engine from a horse-wheel.

AUDUBON'S LILY REDISCOVERED.

By Professor SAMUEL LOCKWOOD.

DISCUSSING the varied exhibits made of the natural sciences in the late Exposition at Philadelphia, *Forest and Stream* pays a high compliment to a collection of water-color paintings of "The Birds of New Jersey." These paintings are the work of G. B. Hardenbergh, a youth in New Brunswick, who, having heard, in the Rutgers College Grammar-School, a course of lectures on birds, by the writer of this, became at once an enthusiast, and, with the spirit of a devotee, gave himself up to the study of birds in their native haunts. By wood and stream, in all seasons, the young artist naturalist watches his subject, learns its habits, gets its attitudes, then

shoots it, and, in his study, with a knowledge of all its *posituræ*, produces a portrait that sparkles with active life. The figures are Audubon-like, of life-size, and every one is strikingly natural. And the trees and plants, too, are so accurate that any botanist can, at a glance, identify the species. Each picture has the Flemish peculiarity of scrupulous attention to details, being, in its own way, a bit of rigidly realistic art. All this commends the work especially to the naturalist, and is much in the spirit of the famous Audubon. And, joined to the youthfulness of the artist, it was just this realistic truthfulness which made these simple bird-pictures of New Jersey so attractive at the great Centennial show.

But, can we not see an intimate relation between this æsthetical outcome of the artist and his own ethical inwardness? All this tender care for the details, this high regard for the truthful narration of the pictorial story, comes of the scientific conscience. Its processes are directed by the religiosity of good, honest work; and thus form is given to what may be called, as its resultant, the conscientiousness of art.

And yet, strange to say, this charming naturalist and artist, this, so to speak, consecrated student of Nature in her own haunts, whom so long every one, both at home and abroad, lauded for his fidelity to Nature, has of late been under a cloud. Yes, the truthfulness of even Audubon stands under attaint of both ornithologists and botanists. Let us adduce the specifications.

Our boyish delight still lingers in memory over the reading of this wonderful man's account of his first sight of that bird whose celebrity, unhappily, has given place of late to an undesirable notoriety. In a burst of enthusiasm, in which the love of Nature and of country mingled, he called it "the bird of Washington," and that Science, to the end of time, should do the same, he named it *Haliaëtus Washingtonii*. Thus stands his behest to science in his "Ornithological Biography," vol. i., p. 58:

"He first saw it on the Upper Mississippi, in February, 1814. A few years after, he met with a pair near the Ohio River, in Kentucky, which had built their nest on a range of high cliffs. Two years after the discovery of the nest, he killed a male, which was the subject of his description. After this he saw two other pairs near the Ohio River. It seems not to have been seen by any other ornithologist. Though this bird is admitted as a species on the authority of Audubon, many ornithologists do not regard it as such; and, from Audubon's own testimony, there seems sufficient ground for doubting the validity of the species."—("American Cyclopædia," revised edition, article "Eagle.")

In one of those delightful "Letters on Ornithology," by Dr. Coues, now appearing in the Chicago *Field* (Letter IX., on the "Hawks"), occur these words:

"While we have gray eagles, and black eagles, and eagles without stint, my word for it, reader, this eagle business is about done to death. Let me beg you

not to publish the next eagle you kill. Eagle-stories are almost always 'fishy.' As to the number of different kinds of eagles in this country, believe me when I assure you that there never have been but *two* species discovered in all the length and breadth of this country. That famous 'bird of Washington' was a myth. Either Audubon was mistaken, or else, as some do not hesitate to affirm roundly, he lied about it. The two species are, the golden eagle (*Aquila chrysaëtos*), and the bald eagle (*Haliaëtus leucocephalus*).

This, surely, is somewhat terrific, and would indicate, in this instance, that truthfulness, the bright particular flower in a man's character, was badly wilted. As Patrick would say, "it does'n't become the loikes of us to talk back; and maybe it cowes us, just, to be found in disagramint with the great bird-doctor, who is possissed of the aridition of all the fowls that iver was, sure." So we will not openly differ with this accomplished man; and will even, like a devout Moslem, leave Audubon to those stern ladies known as the Fates, and thus will hasten to another instance in which, perhaps, even a lady may come to the rescue of the reputation of this remarkable naturalist.

If possible, Audubon has suffered worse at the hands of the botanists. From these gentlemen the famous student of the woods and fields has received a snub of the shabby-genteel sort, and of the most persistent character. In his "Birds of the South," and with his usual love of fidelity to particulars, as indicating the plant habitat, or surrounding, Audubon figured a yellow water-lily—not that very ordinary flower, the *Nuphar advena*, the spatter-dock, or yellow pond-lily, so common from Canada to Florida, but a real close cousin to *Nymphæa odorata*, our delightful, sweet-scented water-lily. Beholding it with his own eyes, the great painter put it into one of his glorious bird-pictures, and, having given the portrait of his floral beauty, he also named it *Nymphæa lutea*, or, in plain English, the yellow water-lily. But this pretty flower had never been seen by the botanists; and so, forsooth, the thing was absolutely ignored—treated as a pretty fable, a bit of art extravagance. Art, like history, may have its anachronisms, but the real artist, though he err, cannot lie. So thoroughly was that *Nymphæa lutea* snubbed, that it would have been as much as a poor mortal's reputation was worth to have mentioned credence in the thing in the hearing of sober Science. One might look in vain in any botany of the South for Audubon's yellow water-lily. Not a word can you find in Darbey's "Botany of the Southern States;" and the same ominous silence pervades that later and more pretentious work, Chapman's "Flora of the Southern States." This luckless lily of Audubon is scientifically tabooed. Luckless, was it said? Well, this abjured beauty of the good man has fallen into luck at last. When neither sought nor expected, a species of poetic justice has lately been reached; for, in the person of a lady, learned in such lore, we have "a Daniel come to judgment." Last summer, in Florida, Mrs. Mary Treat rediscovered the long-lost flower

of Audubon. Yes, there it was blooming in those semi-tropical waters, and, from its golden chalice, this excellent lady drank the exquisite pleasure of a scientific discovery, and, sweeter still, the privilege that she could bid pass away that cloud of incredulity of over a generation of years. In fact, it was communicated to that Nestor of American botanists, Prof. Gray, and was duly acknowledged. It was truly the long-ignored *Nymphæa lutea*—Audubon's yellow water-lily. And, more than this, this deported beauty, through our modern Portia's zeal, is to be introduced to the best botanic circles of the world. Mrs. Treat has provided a liberal stock for the botanic garden at Harvard; and the curator, Prof. Sargent, is giving them careful and skilled culture, and is also supplying the gardens of Europe with specimens. Among the botanists, then, Audubon and his beautiful water-lily today stand quoted above par. Whether the "bird of Washington" is to reappear, and set this early ornithologist right with the modern bird-men, perhaps may hardly admit of a hope. That Audubon, like Wilson and the rest, did sometimes err in the diagnosis of his species, was easily possible; that he could lie, we think, was impossible. Much work of these earlier students has had to be done over again, and, as Dr. Coues has shown, this is emphatically true of the *Falconidæ*, or diurnal birds of prey. Very radical undoing has been needed of the work done on the eagles. Lately, we had at our very doors not less than three notable eagles—the black eagle, the gray eagle, and the bald eagle. But more thorough and skillful work has eliminated two out of these three species by showing that the black was the young, the gray the middle-aged, and the bald the mature, or adult stage, all of one and the same species, namely, the *Haliaëtus leucocephalus*—the bald eagle. We would like to see some condonement for the long ignoring of that Southern lily. If it were scientifically orthodox to rechristen that rediscovered flower, we would have its history crystallized in a new specific name, *Nymphæa Audubonii*, which, after so long incredulity, would be doing the bonny thing; and thus the yellow water-lily would dot, with golden memories of the gentle enthusiast, Audubon, the waters of the river of time.

THE PLANT-EATERS OF NORTH AMERICA.

By Professor SANBORN TENNEY.

THERE may, perhaps, be a question in the minds of some, or even of many, as to what animals are absolutely the most useful to man; but there can be no question that those which furnish him with milk and flesh for food, wool and leather for clothing, and which bear his burdens and draw his loads, have very high claims to this rank.

The deer, the antelopes, the sheep and goats, and the oxen, are indeed very intimately connected with our comforts, and even with our luxuries. And the North American representatives of these useful animals deserve our careful attention and consideration; for they are more intimately connected with our welfare as a nation than we yet fully appreciate or even understand. As all of our domestic sheep and cattle have come from wild species, so in the future we are to draw from the same sources some of the most valuable grazing animals that are to stock the pastures and farm-yards of the great farming-regions of this vast country. And we have several kinds of these animals now wild on the plains and in the forests that ought to be added to our domestic flocks and herds; and intelligent legislation should at once be inaugurated to secure this result, which is intimately connected with the welfare of every person on this continent.

Of deer there are in North America perhaps eight species: the black-tailed deer of the Pacific coast; the mule-deer, and the white-tailed deer, of the Upper Missouri region and westward; the common deer of the United States east of the Missouri; the wapiti of the northern and northwestern portions of the United States; one or two species of reindeer; and the moose of the northern portion of the continent.

The moose (*Alce Americanus*, Fig. 1) is the largest member of the deer family, equaling a good-sized horse in bulk, and having very long legs; and the male has very long and broad antlers, which in some instances weigh as much as seventy pounds or more. Its muzzle is exceedingly large and long, its ears long and hairy, its neck short and thick, and the latter and the shoulders covered by a mane, and the throat with long hair. The general color is a grayish brown, and the hair is very coarse and brittle. In its movements the moose appears quite awkward, but it is able to make very great speed, striding along without apparent effort over fallen trees, fences, and other obstructions, which would be serious obstacles in the way of most, if not all, of our domestic animals. The moose is still common in the unsettled parts of Maine and Northern New York, and thence northward toward the frozen regions. In the winter it keeps mainly on the wooded hill-sides; and at this time many of them stay in what the hunters term "yards." These are large tracts of ground over which the snow has been trodden hard by the moose, the lighter and untrodden snow forming a wall around the yard. There are generally in each of these yards one male and one female, and one or two fawns. They feed upon the bushes and the saplings that may be growing in the yard, and even peel off and eat all the bark from the hard-wood trees up as high as they can reach. They are especially fond of the birch, the moose-wood, and the poplar.

In the summer the moose frequents lakes and rivers. Here, by

going into the water, it escapes the attacks of the troublesome flies, and avoids injuring its antlers, which at this time are growing, and are very tender.

FIG. 1.—THE MOOSE (*Alce Americanus*).

The antlers are the most curious things, perhaps, in the structure of these animals. As already stated, they are found only on the males. They are shed annually, in the month of December; in some cases, however, they are carried till the following March. The first year the antlers are merely short knobs; the second year they are four or five inches long, with a single point; the third year about nine inches long; the fourth year they become broad with a brow-antler and several points, and about the fifth year they reach their maximum size. It is a matter of wonder that the enormous horns of these animals grow in about two months! They begin to appear about the latter part of March, or early in April, and in June or July they are full-grown for the season. While growing, they are invested with a skin which is covered with a sort of velvet-like pile; and this skin is nourished by a system of blood-vessels. When they have attained their full growth for the season, the skin peels off, and leaves the antlers at first perfectly white, but exposure soon turns them brown.

The female produces her young in May; at the first birth there is

only one fawn, but afterward two, and it is believed by the hunters that these twins are always one male and one female. The moose is hunted at the yards, and also pursued with dogs until it is fatigued and overtaken; and it is also shot on the lake-shores and river-margins, in the early autumn, by moonlight. The flesh of the moose, though rather coarse, is highly prized as food by many, and is a very good substitute for beef. The nose and the tongue are regarded as great delicacies. The marrow from the shank-bones is used by the hunters to spread upon their bread and eaten as butter.

It may be stated here that our moose is so nearly like the great elk of the northern part of Europe, that there is still perhaps a question whether the two are of one species. A fossil elk has been found in the marl beneath the peat-bogs of Ireland, which is of an entirely different species from any now living. This fossil elk was ten feet high to the top of the horns, whose tips are ten feet apart!

People generally think of the reindeer only as an inhabitant of the cold portions of Europe. But North America has at least one species of reindeer, although it is more generally called caribou. The woodland caribou, or reindeer (*Rangifer caribou*), of New Brunswick, Maine, and westward to Lake Superior, is thought by some to be identical with the reindeer of Lapland. The barren-ground caribou, or reindeer (*R. Groenlandicus*), is found in the arctic regions beyond the limits of trees, and may be only a variety of the former.

Unlike the other deer, the reindeer have the horns present on both sexes. The horns are palmated only at the tip, but, like those of all other deer, are shed and renewed periodically. The history of the reindeer of Lapland is well known, and from that history we learn how useful our own species may yet be made. As is well known, the Laplanders have large herds of these animals, and use them for beasts of burden and for draught, their milk and flesh for food, their skins for clothing and for covering their sledges. The reindeer is a very hardy animal, and draws the sledge of its owner with great speed. In one of the palaces in Sweden there is a picture of one of these animals, which is preserved with great care, from the fact that the animal from which it was painted drew the sledge of an officer, with important dispatches, the distance of eight hundred miles in forty-eight hours!

The caribou or American reindeer (Fig. 2) is considerably larger than the common deer, now so often seen in our parks. Its color is deep brown in summer and grayish in winter. In the winter this animal stays in the swamps, much of the time, and feeds mainly on the mosses and lichens that hang from the trees and bushes, but in early spring it retires to the hill-sides and feeds upon the buds and twigs. Like its European relation, it is very fleet of foot, trotting, or galloping, or leaping, with the greatest ease; and it is also capable of great endurance. For more than a week hunters have followed a

caribou before they could get near enough to shoot it. When attacked by dogs it stands at bay, and then falls an easy victim to the hunters. In the regions far to the north, where the caribou is plentiful, these animals move in herds from ten to a hundred or more. When in good condition the male caribou has a layer of fat on the back and rump two or three inches in thickness. The flesh is an excellent article of food, being tender and of good flavor. The skin when properly

FIG. 2.—THE AMERICAN REINDEER, OR CARIBOU (*Rangifer caribou*).

dressed forms one of the best articles for clothing to be worn in the cold regions. A suit made of the dressed skins of the caribou is so warm that it is said that the person wearing one of these suits and also provided with a blanket of the same material, may bivouac on the snow not only with safety but with comfort even in the intense cold of an arctic winter's night.

The common deer of Eastern North America, generally known as the Virginia deer (*Cervus Virginianus*, Fig. 3), is one of the most graceful and one of the most beautiful of all the deer family. It is now so common in parks that almost every one is familiar with it as it appears in this state of semi-domestication. But no one gets the best idea of this splendid animal who does not see it as it appears in the wild state, either in the forest or on the plains. Here when startled it bounds away with the most incredible velocity, and he who would bring it down must have a quick hand and steady nerve. This deer attains a weight of about two hundred pounds. The color is light brown in summer and grayish in winter, the under part of the throat and tail being always white. The food of this animal is exceedingly various. The tender grasses constitute its principal food in

summer, except in those regions, as in many parts of the South, where the deer can gain access to the fields of young wheat, oats, or other grain. In the early autumn it adds berries of various sorts to its bill of fare, and later still nuts and acorns; and in winter it feeds upon almost all kinds of buds and tender twigs, as well as upon various kinds of the more hardy herbs. The males are in excellent condition from August to November, and the females from November to January. The antlers are fully grown in July or August, and remain till the next January, when they are shed. The males engage in severe

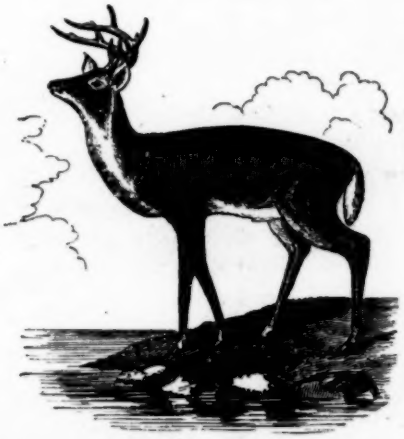


FIG. 3.—THE VIRGINIA DEER (*Cervus Virginianus*).

contests with one another, and in some of these contests they get their horns or antlers interlocked, so that they cannot separate them, and the combatants at length perish from starvation and exhaustion. In some cases the antlers are interlocked so firmly that even a strong man cannot separate them, and Audubon mentions one case where three pairs of antlers were thus united. The flesh of this deer, as is well known, is tender and juicy, and has an excellent flavor. This fact, and the love of the excitement of the chase, have caused this animal to be extensively hunted. At the same time our forests have been disappearing, thus affording them less protection, so that the numbers of the common deer are far less than twenty-five years ago. It will require rigid legislation to keep these animals from entirely disappearing from many parts of our country where a deer-hunt is still possible.

Next to the moose, the wapiti or American elk (*Cervus Canadensis*) is the largest deer in North America. It is nearly as large as a horse, and its horns are the most magnificent to be found in the whole deer family, being five or six feet long and much branched. In some cases antlers of this species have been secured which were so long that when standing on their tips a man could walk upright through

the arch thus formed. Although this deer still lingers in the mountainous regions of Pennsylvania, and perhaps in a few other places in the eastern part of our country, it is confined mainly to the western and northwestern portions of North America, and south of the fifty-seventh parallel of latitude. In some cases it is found in large herds,

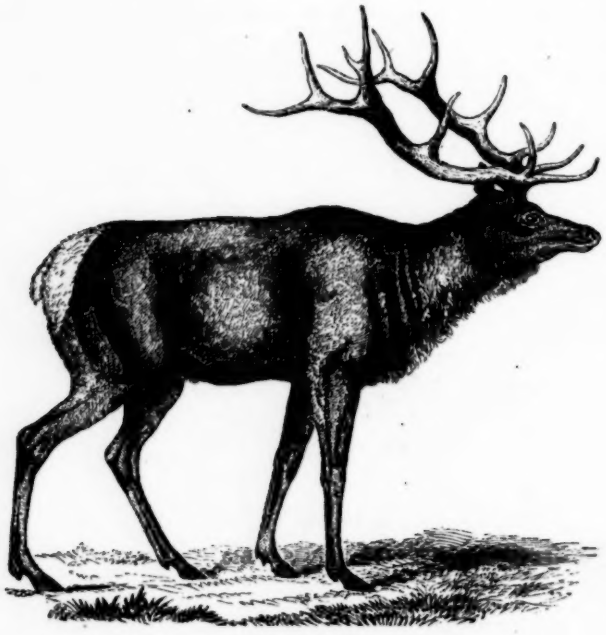


FIG. 4.—THE ELK, OR WAPITI (*Cervus Canadensis*).

all the members of which follow one of the males which is their leader, and whose movements they more or less closely imitate. The color of the wapiti is grayish in winter, and chestnut-red in summer. This deer is the analogue of the stag or red deer (*C. elephas*) of Europe, and was formerly regarded as identical with the latter; but it is a very much larger animal than its European relation, and is in every way a distinct species.

The antelopes differ from all the deer in having their horns permanent and hollow, and, like a sheath, covering a conical process of the frontal bone. In this respect the antelopes are like sheep, goats, and oxen. The antelopes have the horns round, curved, ringed, or wrinkled, and always black. There are many species of antelopes, no less than ninety having been described. Of these, two are found in North America, two in Europe, and all the rest in Asia and Africa.

Our most interesting species of antelope is the prong-horn (*Antilocapra Americana*, Fig. 5) of the western portions of North America. It is about the size of the Virginia deer, and is covered with coarse,

thick hair. Its color above is yellowish-brown; the rump and under parts white; the horns, hoofs, and the naked part of the nose, black. The white hair covering the rump is very long, and seems to be under the perfect control of the animal, and is at once made to stand erect when he is in the least excited; and it is wonderful to see this patch of hair rise and fall with his varying emotions. About half-way up the horns of the adult there is a branch or prong, and from this fact the animal gets its popular name.

The prong-horn is often seen alone, more frequently perhaps there are several together, and in some cases herds of one or two hundred

FIG. 5.—THE PRONG-HORN ANTELOPE (*Antilocapra Americana*).

are seen. It is not an uncommon thing for the traveler on the Pacific Railway to see several of these beautiful animals while he is crossing the Plains. One has been seen to run along for a mile or two parallel with the moving train, as if determined to keep up with it. Its speed is very great, and is only equaled by that of the fleetest of the deer; and hence it is almost useless to pursue it. It is not, however, difficult to secure these animals. They have great curiosity in regard to any objects which they are not accustomed to. The hunters well know this fact, and turn it to their own advantage. When the experienced hunter sees a prong-horn, or a herd of them, he does not pursue them, but keeps his ground, or little by little advances very slowly. The antelope soon advances a little toward him. The hunter waves his handkerchief, or a rag; the animal approaches still nearer and nearer; and in this manner he is soon within easy range of the hunter's rifle. It is stated that the Indians have the habit of lying flat upon their backs, and kicking up their heels with a rag or something fastened to them; and that by this process they entice the prong-horn to within such a distance that they kill it with their bow

and arrow. The flesh of the prong-horn in autumn, when it is in the best condition, is good food, especially if the animal be young.

In May and June the prong-horn brings forth two fawns, which are of a dun-color, and not spotted like the fawns of the deer. For these the mother displays great affection, and defends them with vigor against the attacks of enemies. She is sometimes able to beat off even the wolf; but not always, and hence many of these little creatures are annually destroyed by this hungry animal. The prong-horn, when taken young, is easily tamed. The writer has seen a tame one. It was thoroughly domesticated, and, whatever its wanderings during the day, it returned to the farm-house at night. It allowed itself to be freely handled, even by strangers. It followed the children as they went to school, and then returned to its home again, alone; all showing how easily it can be added to the stock of domestic animals of the farm.

Our other species of antelope looks so much like a goat that it has been named the mountain-goat (*Aplocerus montanus*, Fig. 6). It is about the size of the domestic sheep, and has small, round, slightly

Fig. 6.—Mountain-Goat (*Aplocerus montanus*).

recurved horns, which are ringed at the base, and which are jet-black in color, and polished, and are much like those of the chamois; the body is covered with long, white hair, and there is a long pendent tuft of hair under the chin. This antelope lives on the rugged portions of the Rocky Mountains, and seldom descends into the plains. It leaps from crag to crag, much after the manner of the chamois of the Alps, and in many portions of the mountains is secured with great difficulty. The flesh of this species is rather dry, and is not so highly prized as that of the other animals described in this article. It may be added here that the hair, or covering of the body, is of two kinds, the one being long and straight, and the other, which forms a thick, close under-coat, being a sort of fine silk-like wool.

Of sheep there is only one wild species in North America, the Rocky-Mountain sheep, or big-horn (*Ovis montana*, Fig. 7). This animal is of a much larger size than the ordinary domestic sheep, and its horns are of enormous size. A large animal of this species weighs about three hundred pounds. In Siberia there is a wild-sheep, called the argali, which Cuvier believed to be the same as our big-horn. It is certainly very remarkable that there should be only one species

FIG. 7.—THE MOUNTAIN-SHEEP (*Ovis montana*).

of wild-sheep on this continent, and that that one should be confined to our highest system of mountains. The inquiring mind naturally asks, "Whence has this sheep come?" But this question is not easily answered. It may, however, be stated here that Cuvier was inclined to believe that it came from Siberia, and crossed Behring's Straits on the ice.

The Rocky-Mountain sheep lives in flocks, and is exceedingly wild, especially in regions that have been frequented by the hunters; and he who would get a shot at one of these animals has often to make wide *détours*, and always to proceed with the greatest caution. The flesh of this animal is very highly prized, being regarded by some as even better than venison, or ordinary mutton. The hunters tell remarkable stories of the big-horn. They assert that this animal will leap sometimes from high precipices, head foremost, and, striking upon the tips of its enormous horns, bound away on its course as if nothing had happened!

Characteristics belonging to different kinds of animals are some-

times combined in one and the same animal. The musk-ox (Fig. 8) furnishes us with an example of this sort. This animal is in some respects so much like a sheep, and in others so much like an ox, that naturalists have named its genus *Ovibos*, the first part of the word meaning sheep, and the latter part meaning ox. The musk-ox (*Ovibos moschatus*) inhabits the barren ground of North America, and is about the size of a two-years-old heifer. Its horns are close together on the top of the head, whence they curve outward, downward, and thence upward. The body is covered with long pendent hair, and the color is brownish-black. This hair or wool might be made very serviceable in the manufacture of useful fabrics, if it could be obtained in sufficient quantity. The musk-ox lives mostly in herds of a score or more, and, contrary to what we would naturally suppose, it runs with great speed, and climbs rocky hills with facility. The flesh of the young animals is very good, but that of the older ones is too strongly impregnated with musk to be palatable to white men, although the Indians and Esquimaux may not seriously object to it.

FIG. 8.—THE MUSK-OX (*Ovibos moschatus*).

It is much to be regretted that the musk-ox is so rarely preserved in our museums. It is exceeding difficult to secure a specimen, as almost every one which is killed by the natives is immediately devoured by them. The food of this animal consists of grasses in the summer and lichens in the winter, the latter being obtained by scraping the snow from the ground. On this food they keep in remarkably good condition. It may be added here that only one species of musk-ox is now living; although their fossil remains show us that in the past there have been other species of this animal, and in other parts of the world than America.

It is interesting to see how the same idea under specifically different forms is represented in the animal kingdom in the different portions of the earth. Take the idea which finds its expression in the ox, for example. In the southern part of Africa we find the ox in the

form of the Cape buffalo, a very ferocious animal, with horns so wide that they nearly cover the forehead; in India, the arni, whose enormous horns are ten feet apart from tip to tip; in the forests of Lithuania and of the Caucasus, the aurochs, an ox related more or less closely to our wild species; in Tartary, the grunting cow or yak, which is smaller than any of the preceding, and which has a long mane upon the back, whose tail much resembles that of a horse, and whose grunting is similar to that of a hog. And in North America we find the American buffalo (*Bos Americanus*, Fig. 9),

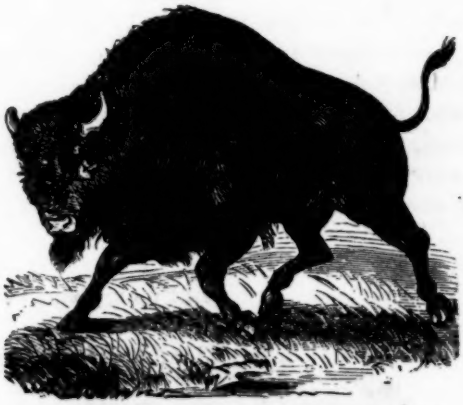


FIG. 9.—THE BISON OR BUFFALO (*Bos Americanus*).

the largest quadruped on this continent. This animal once inhabited nearly all of North America, except the cold regions of the north; but it is now confined mainly to the great Western plains, where, notwithstanding the immense havoc made among their numbers, both by Indians and white men, they still exist in numbers that almost defy computation, in some places covering the plains in every direction as far as the eye can reach. The buffalo is as large as a good-sized domestic ox, and has a large head which is carried close to the ground, a broad forehead, a broad, full chest, a large hump between the shoulders, narrow loins, and rather slender legs. The horns are set far apart, are large at the base, and taper suddenly to a sharp point. The buffalo is covered with a thick coat of hair, that upon the head, neck, and shoulders, being very long and shaggy. The horns and hoofs are black. Perhaps there is no grander sight to be witnessed among the larger animals than to see one of the immense herds of these animals, when under good headway, sweep by—if only the observer has a safe standing-place.

When the buffalo is moving rapidly, it progresses by an awkward canter or gallop, and it requires a good horse and an expert rider to keep up with it. The hunting of the buffalo is one of the most exciting and at times one of the most dangerous sports, if such it may

be called, in which the visitor to the great Western plains can engage. Unless shot through the heart or some other vital part, this animal is not easily brought down. When the animal is only wounded it becomes very furious, and, if its pursuer be on foot, it at once attacks him, and the hunter has all he can do to save himself from destruction. Nor is he always safe even if he be mounted, unless he can manage to keep out of the way of the infuriated animal, for he ferociously attacks both horse and rider.

Buffaloes wander much from one region to another in search of the best pasturage, and of water, salt, or saline springs. In the winter they move southward, and in spring return again to the north. Their deep and well-trodden paths traverse the plains for hundreds of miles. Vast numbers are destroyed during their spring and autumnal migrations. Many perish from starvation; those that get weak and are left behind, are harassed and at length devoured by wolves. Sometimes the vast herds attempt to cross the rivers upon the ice, and, when they are crowded together, the ice gives way and they perish in the cold waters.

The male buffaloes have terrible combats. The young are born in April and May, and there is generally only one at a birth. The young are in constant danger from the wolves.

The buffalo is easily domesticated, and should be added to our stock of domestic cattle. The flesh of the wild ones is extensively used for food, and is regarded with much favor; and we already know enough to convince us that the flavor of their flesh would be improved when they are fully under the dominion of man. Experiments show that the males make excellent oxen, and that they are stronger and swifter of foot than the ordinary oxen; and, when we consider that it takes the milk of two domestic cows to properly nourish one buffalo-calf, we may safely conclude that the females will make excellent domestic cows.

The buffalo was once common over most of North America west of the Hudson River. In the Carolinas they were found even on the seaboard. But, like the red man, they have fled westward, before the advance of civilization, and are still fleeing. Their natural feeding-grounds become cultivated fields. Enemies are constantly on their track. Man hunts them for their valuable skin and for their flesh. Vast numbers are killed yearly that civilized man may feed upon their tongues. Wolves and bears lurk in ambush to snatch away the young, and more openly to wage a constant warfare against the sick and disabled members of the herd. So that, notwithstanding their vast numbers, the day is not far distant when the buffalo will be as rare a sight on the Plains as the wapiti and the moose are now in our Northern forests.

THE SCIENCE *vs.* THE ART OF CHEMISTRY.

By IRA REMSEN.

THE attitude of the world in general toward chemistry is peculiar, and, as this paper is intended to show, it is not what it ought to be. This is due in turn to a peculiarity of the science itself, which distinguishes it from most other sciences. We refer to its close connection with matters of every-day experience, and of practical importance. It is unnecessary to dilate here upon this close connection. Every one who has any conception whatever of chemistry recognizes it to a greater or less extent. But, owing to this close connection, the unscientific world has grown into the habit of considering the practical problems as the problems *par excellence* of chemistry; and, having once recognized *some* object of the science, they inquire no further, and hence they fail to recognize its most important and only legitimate object.

In this respect chemistry as well as physics is unfortunate; though at the present day physics has an advantage over chemistry. Time was when the world looked upon physics also as mainly a practical science; but, of late, by the efforts of gentlemen of high standing, the attention of the people has been drawn to some of the higher problems of the science, and these have been rendered intensely interesting to every thinking being. Some of the grander results of physical investigation have also become familiar to the world, and have served to increase the respect for the science. The great truths of the conservation of energy and the transformations of energy; the application of the spectroscope to the investigation of heavenly as well as earthly bodies; the undulatory theory of heat; the nature of sound, and the beautiful relations of sounds to each other—these are all matters with which the world is fast growing familiar; and the popular discussion of these subjects is doing something, perhaps a great deal, to elevate mankind above that condition of superstition and darkness which still is the portion of most of the world. The great generalizations of science are ennobling, and, in the exercise which they afford the intellect, are productive of happiness of a very high order. Whatever good we may recognize, as having been effected by the practical application of electricity, heat, and other natural agents, to the satisfaction of the wants of man—and the good is undoubtedly great—an infinitely greater good springs from the dissemination of the immortal truths of physics. But the latter good is quietly effected; it consists in a growth of the ideas of the world, and thus contributes to the growth of manhood. We do not always recognize it, but it is ever present. With the growth of ideas concerning the physical universe, the ideas concerning the Creator of the universe must grow

larger, broader, grander, and we must worship with a truer adoration, and a feeling of more perfect reverence.

If we turn again to chemistry, we shall see that while its importance is almost universally recognized; while the number of those who devote themselves to its study is increasing every year; while immense sums of money are yearly spent for the building and support of palatial laboratories; while the press, recognizing the popular appreciation of the science, furnishes, in its own peculiar way, brief records of its advance—still we can point to very little connected with chemistry which, for its elevating influence upon mankind, can be compared with the great physical truths above referred to. That which is caught at and served up for the public is taken from the lower portions of the science, while the higher portions pass on, scarcely if ever coming in contact with the populace. The public knows when a new dye is discovered; it knows when the poison has been found in some strange stomach; it knows when a new milk for babes has been concocted; it knows when precious metals have been detected in the depths of the earth; it knows all these things because it is promptly informed in regard to them; and it is right and good that the information should be given, and that these things should be known. It is plain, however, that a thousand dyes might be discovered; that a thousand murderers might be brought to justice through the aid of the chemist; that varieties innumerable of milk for babes might be concocted; or that mines upon mines of gold might be unearthed without the slightest ennobling or elevating influence being exerted upon the mass of mankind. All of these things would be valuable—undoubtedly—but their value would be of a very material kind. It is certain that this material value is that which is most easily recognized, which appeals most directly to the public; and hence plainly, in the public mind, the importance of chemistry is measured by the standards of this value. The reputations of chemists, too, depend upon the greater or less extent to which they devote themselves to practical questions. He who is frequently on the stand to testify in regard to cases of poisoning; he who succeeds in presenting to the world some new compound which can be used practically; he who detects impurities in our food or tells us of poisons where their presence must be of importance to us—this man is, to the public, *the chemist*. Ask ninety-nine men out of a hundred what a chemist is, and they will give a definition of one who practises the art of chemistry, rather than of one who is devoted to the science of chemistry.

This statement is true, whether we speak of the mass of mankind, or of educated and even professional men. The reputation of the science, at the present time, is such that few men conceive of the true science independently of the art of chemistry. This is true, further, not alone in this country, but in Germany, which may rightly be called the seat of chemistry—with this difference, however: In Germany

the true scientific spirit is so deeply imbedded in the educated mind, that a subject which has a practical side is apt to be looked upon in a disrespectful manner; and so it happens that those who ought to know better are inclined to speak contemptuously of chemistry, simply because they accept the popular idea of the science as the true one, not stopping to ask whether there is anything higher in the subject than that which the public recognizes. An anecdote which illustrates this matter clearly may not be out of place here. Two students at a German university, one a philologist, the other a chemist, were conversing, on the eve of their examination for the degree of doctor of philosophy. The philologist asked, "What is the subject of your thesis?" The chemist answered: "Piperic acid; I have been working on the subject for a year and a half." When it was further stated, in reply to inquiries, that this acid could not, so far as was known, be used for any practical purpose, the philologist was loud in his expressions of pity for one who could work a year and a half without accomplishing something which would tend directly to improve the material condition of our race. A counter-question in regard to the subject of the thesis of the philologist elicited the answer: "My subject is an exceedingly interesting one; I have already written nearly a hundred pages on it and have not yet finished: it is *the preposition* AD *in Tacitus*." It is needless to add that he was unable to state to what practical use the preposition *ad* could be put. The condition of mind toward chemistry which this young man thus betrayed is that which we should most frequently find in educated as well as uneducated men in this and other countries. We would not throw ridicule upon the enthusiasm displayed—we admire it; but we ask to be allowed to have a similar enthusiasm for our prepositions.

We have thus found the chief cause for the idea commonly held in regard to the nature of chemistry to be that peculiarity of chemistry among the sciences which gives it its close connection with practical matters. It has already been remarked that it is right that this portion of chemistry should be recognized and appreciated. This recognition and appreciation should be encouraged, but not to such an extent as to sacrifice any appreciation which it is possible to awaken for the higher portions of the science.

There is another direct cause for the popular conception of chemistry, growing out of the more general and indirect cause already considered. This consists in bad attempts to present the truths of the science to the people. The popular lectures on chemistry which are usually delivered are not scientific lectures; they are frequently utterly lacking in everything that characterizes scientific method; and they leave no further impression on the minds of the hearers than that chemistry is a subject which enables men with the requisite degree of skill to become successful showmen. Though the lecturer is perhaps more respected, still the character of the respect which he has

called forth is akin to that called forth by any clever trickster. It is unfortunate that experiments, originally devised for the purpose of teaching facts, should have come to be employed simply for the sake of their æsthetic effects. There can certainly be no harm in making an experiment a thing of beauty, so long as its real object is not by this means interfered with; indeed, this may be advisable, in order more strongly to impress upon the minds of the hearers the facts which are to be taught, but the tendency is very strong toward the condition above described: the science is made to serve the purposes of showmen, and the rabble shout the more, the greater the display. Those who serve up this class of lectures are doing positive harm by belittling the science whose name they profane; and they are also doing negative harm by failing to make use of the opportunities afforded them to draw the minds of men upward to higher conceptions, and thus of elevating mankind. They neither recognize the science nor the art of chemistry, but by their actions teach that it is a pastime of no particular value.

In the foregoing we have drawn a line between the science and the art of chemistry. The character of the art is perfectly plain to every one. He who analyzes substances in order to decide questions solely of practical importance; who examines the properties of substances solely with a view of determining the practical uses to which these substances can be put; whose only problem relates to the applications of the truths of chemistry to the uses of man—*he* practises the art of chemistry.

But it is time to inquire what the science is, and what its relation to the art is. A science is a collection of principles, well established, applying to a certain class of phenomena. The science of chemistry is that particular science which treats of the action of bodies upon each other, in so far as this action causes a change in the composition of the bodies. All the so-called natural laws which govern this kind of action belong legitimately to the field of chemistry. The science is, strictly speaking, a part of that broader science which treats of the action of matter upon matter, viz., physics; but it is usual to consider the two as separate sciences. Its first object is to determine the laws of combination and decomposition of bodies, and its state of perfection will be reached when so much is known concerning these laws that we shall be able in every case to foretell what changes will ensue when two or more bodies are brought together, or when certain influences are brought to bear upon a body. We are so very far from this perfect state at present that we cannot even say what kind of reasoning processes will be necessary to enable us to draw the proper conclusions from given facts. It appears probable, however, that chemistry will gradually develop into a true mathematical science, and that, having reached this state, chemists will determine the orbits of atoms, their rates of motion, their perturbations by methods similar

to those so long employed in studying the problems of astronomy. Although we are far from the perfect state of the science, still every advance made in it is a step toward the end. From time to time material enough is collected to enable some one to make a comprehensive generalization. These generalizations we admire, but we sometimes forget that they never could have been made had not a myriad of workers from day to day furnished the material; themselves often unconscious of the importance of the real work they were doing, but believing that every fact established, however insignificant in itself, every error of previous observers, however slight, corrected, would at some time serve a purpose in the growth of the science. Dalton's law of multiple proportions; the law of Dulong and Petit connecting the specific heat and the atomic weight of the elements; Avogadro's hypothesis relating to the connection between molecular weights and the volumes of gaseous compounds, would still have been of the future, had it not been for the efforts of a great many scientific workers, contributing their mites day by day.

Though we thus recognize a growth of the science of chemistry, entirely independent of any practical applications of its facts, it is of course true that the latter follow closely in the footsteps of the former. When, then, we rejoice in any useful application, let us remember that it could never have been made had the science itself, as a science, not advanced.

It happens in this country particularly that a man may both practise the art of chemistry and at the same time be a worker in the field of scientific chemistry. This is due to the fact that it is necessary for the men to live, and there are very few positions in the country which enable their incumbents to devote themselves to the pure science of chemistry without obliging them at the same time to look for additional means of support to that furnished by the positions themselves. This additional means of support can usually be found most readily in the practice of the art of chemistry. Too often, time that could and would be devoted to grappling with the problems of the science is given up to the art in order to keep the purse supplied. Every properly-constituted scientific man, however, who is obliged to so apply his powers as to bring himself immediate and material rewards, feels that he is doing something which he would rather not do, and that, by applying himself to his science proper, he could in the end be of much more service to the world. It is apt to be the case, too, that he who begins to slight the science and to favor the art will at last entirely sacrifice the former for the latter, and we see too many teachers of chemistry in this country at the present day who are devoting their time to the art rather than to the science of chemistry; a circumstance which has the most pernicious effect upon the growth of the science among us, for the students who are placed under the influences mentioned are not stimulated, as they should be, to con-

sider the higher questions of the science, but go out into the world only to keep alive the popular and erroneous idea concerning the nature of chemistry.

Finally, if we have correctly represented the attitude of the world toward chemistry, and correctly stated the causes of this attitude, it is plain that the world is not to be blamed, but rather, if fault is to be found, it must be with the chemists themselves. To them we must look for deliverance. They may by united efforts bring about the desired changes. But how?

Two general methods may be indicated. In the first place, the teaching of chemistry must be of a higher order than it is at present. In some of the higher institutions of learning students must be carried through strictly scientific courses; they must be brought face to face with the great questions of the science, and shown how to work at the solution of existing problems; and they must go forth with high and true conceptions concerning their science, prepared to influence those with whom they come in contact, and to give them, too, correct ideas. A great deal can thus be done in the right direction by a single strong man teaching properly, and the influence is very quickly felt. We need only refer to the influence of Agassiz on the science of zoölogy in this country, to show what results may be reached by a single man who is working in the proper way. A change in the methods of teaching in our higher institutions of learning, then, is the chief thing to which we are to look for an improvement in the popular conception of our science. But there is another means at our command which is very rarely taken advantage of by scientific chemists. This consists in popular presentations of the higher truths of the science, either in the form of lectures or of articles in magazines which are read by the public. A great deal of good can be accomplished in this way, if the work is properly done. There are chapters of great inherent interest treating of matter which belongs in the domain of the science of chemistry, and these are rarely alluded to in popular lectures or articles. If more stress were laid upon such subjects, and less upon the merely practical portions of the science, something would be done in the way of drawing the attention of the public toward the higher questions, and thus that good influence which was above referred to as resulting from popular discussions of the great truths of physics would also be felt, to some extent, in connection with chemistry. Thus, too, there would gradually grow up a respect for the science as well as for the art of chemistry.

VITAL STATISTICS.

By CHARLES P. RUSSEL, M. D.

NO subject of scientific research has within the present century received more earnest attention from thoughtful minds than that of statistics. None, moreover, is more worthy of investigation or fruitful of more satisfactory practical results to humanity. It must be confessed that careless or dishonest observers occasionally misconstrue or misinterpret the significance of statistics; but the same is equally the case with all facts. There can be no doubt that certain truths are demonstrable by figures, and that we must accept almost without qualification the old adage that "figures cannot lie." We should not confound with statistics themselves the erroneous deductions drawn from them so frequently.

Among the various divisions of statistics the one which relates more particularly to birth, marriage, and death, must always occupy the most prominent place in human interest. It is this to which the expression *vital statistics* has appropriately been applied, and as "self-preservation is the first law of Nature," so if by a study of this science we can, so to speak, grapple with Death himself and retard his course even for a time, we may assuredly congratulate mankind. This science, as its name implies, takes cognizance of the essential circumstances of human existence, while it must obviously possess inherent and intimate relations with other branches of statistical inquiry, viz., those of morals, industrial pursuits, customs and modes of life, material prosperity, peculiarities of soil and climate, domestic economy, and even political tendencies and events.

If the deductions gained from vital statistics are to be of value in the preservation of life, those facts which bear particularly upon the preventable causes of death must naturally claim our more immediate consideration. The subject of mortuary statistics is, indeed, one of profound interest. All civilized nations have finally recognized its importance, and have by more or less stringent legislative enactments enforced the collection, preservation, and proper arrangement and analysis, of those data which constitute its foundation. It must be acknowledged that even exact figures of mortality do not always indicate *with positive accuracy* prevailing conditions of the public health, especially in the case of affections subject to constant fluctuations of type. They are, however, indices which point unerringly in the right direction, and, as such, they are entitled to our most careful consideration. Moreover, they are our sole means at present for approximate investigation of national disease. We may trust that ere long the concerted action of the entire medical profession will furnish us with a constant knowledge of the comparative prevalence

of all disease. In the United States the want of such a system is in a manner compensated for by the periodical enumeration of causes of death at each national census. Although for obvious reasons such enumeration must be defective, both as regards the actual causes themselves and the number dying within the census year (the returns of 1870 being computed as forty-one per cent. less than the true number), still, the same sources of error and the same elements of truth obtaining, as a rule, in every section, the results of comparisons between different portions of the country contain much less of fallacy and more of fact than might be anticipated. For the last census year, ending June 1, 1870, nearly half a million deaths were collated and appropriately arranged by the Census Bureau, in tables referring both to the country as a whole and to separate States and Territories.

Among our English kinsmen across the Atlantic there has existed for many years a uniform and comprehensive system of death-registration. Thus, within a brief period of the outbreak after an epidemic, its mortuary figures from every quarter reach the central bureau in London, where they are at once systematically tabulated and published. The character of the morbific storm is studied, and its course predicted with almost as much certainty and promptness as each approaching disturbance of the elements is foretold and described in Washington from a comparison of manifold meteorological phenomena. In the same manner, whatever peculiarities may characterize the mortality by sporadic and endemic affections at different seasons, in various portions of the country, are observed and converted into numerical expressions for analytical study.

It is unfortunate for the cause of medical and sanitary science that no similar system has yet been established in this country. In our population of forty-odd millions over seven hundred thousand deaths must have occurred within the last twelve months; and yet, except in the case of our large cities, we are almost as ignorant of our causes of mortality as we are of those which cut off the population of China.

The British system, one applicable to the peculiarities of different populations, was devised by Dr. William Farr, the distinguished medical director of the English Registrar-General's office. A statistical congress, under the auspices of the French Government, was convened in Paris in September, 1855, to consider this subject, and it agreed upon a nomenclature of the causes of death substantially the same as that proposed by Dr. Farr. At another congress held in Vienna, in 1857, a uniform nomenclature and plan of registration for all the European states was determined upon. Dr. Farr's classification of diseases was not so generally adopted; but it has since been making its way in Germany and other portions of Europe. This nosological classification, though by no means perfect, doubtless possesses, in its practical relations to public health, advantages over every system that has preceded it. Its divisions are founded upon the manner in

which diseases of similar type or character affect the population. It will be sufficient to mention its first great class—that of zymotic diseases. This term *zymotic* is derived from a Greek word meaning *ferment*, and has reference to a change analogous to that of fermentation occurring in the blood by the infinite multiplication of disease-germs. Such affections chiefly comprise fevers *par excellence*—the epidemic, endemic, and contagious or infectious disorders—which suddenly attack masses of people, which spring from different sorts of malaria, or from specific communicable poisons; contaminate the atmosphere and water, and decimate in a brief time civil and military communities. We read in sacred history of whole armies having been suddenly swept away, as that of Sennacherib, which, while besieging Jerusalem, lost 185,000 men in a single night under the deadly breath of the destroying angel—a beautiful metaphor, probably, for the swift and invisible blow of the pestilence. It has been well remarked that these diseases distinguish one country from another, one year from another. They have formed epochs in chronology, and, as Niebuhr has shown, "have influenced not only the fall of cities, such as Athens and Florence, but of empires."

This great class of maladies is the index of salubrity; it is this class which varies to the greatest extent in different climates and seasons, which modifies the fatality of other kinds of disease, and which constitutes the principal difference between the health of different peoples and periods.

A general and uniform system of death-registration among nations renders easy what would otherwise be impracticable, viz., constant international exchanges and comparisons, not simply confined to individual affections, but applicable as well to immense groups of cognate diseases. In this manner statistics of mortality assume vast importance, and present for our consideration manifold questions of a physical, social, and political character. They determine the laws which regulate the duration of life; they indicate in what manner those laws have been or are being infringed, and afford bases for calculations materially affecting the interests of mankind. Statistics are far from being the barren array of figures ingeniously and laboriously combined into columns and tables, which some persons are apt to consider them. They constitute rather the ledger of the people, in which, as the merchant in his books, the citizen can read at once all the results of a week, a month, a year, or series of years, and can deduce the profit or the loss which has accrued to the account of vitality, morals, education, wealth, power. And it has been well said that "science has nothing to offer more inviting in speculation than the laws of vitality, the variations of those laws in the two sexes at different ages, and the influence of civilization, occupation, locality, seasons, and other physical agencies, either in generating diseases or in improving the public health."

But, putting aside this broad and philosophic view of the importance of mortuary statistics, it is evident that the application of their deductions must be of great benefit to the physician as a practitioner alone. This was perceived even as far back as the time of Sydenham, who inculcated the doctrine that the treatment of all disease should have a reference not only to the immediate symptoms and to the season, but also to the epidemic constitution of the year and the locality. It has been remarked by a distinguished author that "man is not born, does not live, does not suffer, does not die, in the same manner on all points of the earth. Birth, life, disease, and death, all change with the climate and soil—all are modified by race and nationality." Medicine, with the other natural sciences, has now been obliged to abandon vague hypotheses for truths determined by observation. Numerical expressions are substituted for uncertain and conjectural assertions. Only a limited number of facts are, however, contained within the horizon of a few observers. The determination of the laws of mortality requires a very wide range of observation, and a considerable space of time, in order to eliminate accidental perturbations.

The next important element of vital statistics is that of birth. Man is ushered into existence under natural circumstances almost as impressive as those which circumscribe his duration of life, and which attend its surrender. While tens of thousands are divesting their being of earthly garb, and entering upon their eternal inheritance, still greater numbers are assuming the heritage of life in forms moulded by antecedent events, and stamped with ancestral peculiarities. If, therefore, it be profoundly interesting to contemplate, arrange, and study the multitude of agencies which impel this innumerable caravan of pilgrims toward their destination, it is almost equally instructive to analyze the manifold causes which have contributed to their assembling together. Such particulars, when massed into statistics, become of acknowledged importance to medical and social science. The disparity in the sexes born at different periods, the average number of women bearing twins, triplets, etc., the proportion of offspring from native or foreign progenitors, the ages and occupations of parents, the average number of children produced at different periods of female life and in different seasons, the influence upon reproduction of the relative ages of parents, the reciprocal relations between illegitimacy and modes of living—these and other kindred questions are of deep concern to the human race, and the source of their solution lies in the largest accumulation of facts.

Moreover, the actual number of births occurring in any community each year is indispensable, in conjunction with other factors, for computing the increment of population during years intervening between those of official enumerations, and consequently for the determination of the true death-rate. The remarkable precision with which this

increase may be approximated is exhibited by the London tables, according to which the *estimated* population of that city on April 2, 1871, was 3,247,631; while the *decennial census* completed on the same night gave the number of the inhabitants as 3,251,804—a difference of *only about four thousand in three and a quarter millions*—one almost inappreciable in the calculation of percentages.

To the casual thinker, statistics of marriage might seem of little consequence. But, in fact, the deductions from a review of marriage-returns are of positive value not only to the moral philosopher, but to the political economist as well. The relations of marriage to various industries—to mining, agriculture, trade, commerce—in a word, to the material prosperity of a people—have been well established by statistics. A decided diminution in the marriage-rate of a community within a given period of time is an unerring indication that war or pestilence, or commercial crisis, or other great disturbing force, has rendered the necessaries of life dear, and occupation difficult to procure. The various forms of marriage—the numbers of bachelors, widowers, spinsters, and widows, united in wedlock; the tendency to early or late marriages among certain classes and peoples; the condition of elementary education as indicated by the proportion of men and women capable of signing their names to marriage-documents; the effect of a demand for skilled labor upon the proportion of early marriages; the relations between waste of life and proportions of marriages and births in towns as contrasted with rural districts; the influence of the marriage-rate on morality; the ratio of marriages to births, and its conformity to density and character of population, and to industrial pursuits—all of these considerations furnish assuredly social problems of deep and constantly-increasing importance to civilization.

WORLD-CREATIONS.

By C. C. MERRIMAN.

THE New-World pioneers of the sixteenth century, when they first looked on the sea-worn shores and giant forests of New England, had in reality no compelling reason for believing in the veritable old age of this new-found land. They had no "first order of proof" that the shores were not recently upheaved there for them to land upon, and with the growth of the centuries on them for the trial of the manhood that was soon to reclaim them. But I think those sturdy adventurers, if they stopped at all to consider of scientific doubts, were not long in deciding that the scene before them was conformable to the laws and processes of Nature, and therefore must have been the slow growth of time.

In like manner, the geologist, looking into the bowels of the earth, and finding here and there the remains of a tree or a saurian, presumes that they once lived and grew in the same localities, and were buried and petrified under the rock-grindings of after-ages. But he really has no absolute proof of any such thing. They may have been created in the fossil state and laid away in the strata on the same day the earth was made. But I think the scientist, knowing laws of Nature by which, with sufficiently long periods of time, all these geologic results might have been gradually brought about, is justified in believing that they too were the slow product of Nature and of time.

So we, finding that the world has certainly at some time been subjected to a heat at least sufficient to volatilize nearly every known substance, and that there are laws of Nature by which, through periods of time immensely long, the earth and the planets might have been rolled up from a gaseous nebula and bowled off in their mighty revolutions, have just as much right to say that it was so, as we have to say that the American forests grew, or that the Triassic beds were deposited.

Geology has proved that the earth, up to the primary rocks, was once a molten mass. The crystalline structure of the unstratified rocks compels to this conclusion; for minerals insoluble in water can only become crystallized in large masses by cooling from a state of fusion. If, then, the earth was once an incandescent globe of melted rocks—for everything above the granite beds must then have been in a state of vapor—it is not unreasonable to suppose that it may have existed prior to that time in a still more highly-heated condition—even volatilized, and diffused through space as rare and attenuated gases; for this is the condition which all matter assumes under sufficient degrees of heat. In fact, we must either suppose that the earth was created as a fiery liquid globe, for which we have no warrant, or we must follow back to the time when its vapors were scattered in space, unreflecting and impenetrable to light—when the earth was "without form and void, and darkness was upon the face of the deep."

Let us start, then, with that condition of things which it is now very generally conceded must once have existed—the diffusion of matter in a nebulous form throughout all space. Calculations easily made show that the nebula must have been of extreme tenuity—such that the few grains taken up on the point of a knife-blade must have been expanded to fill several cubic miles. A heat so powerful—for we know of no other force which could thus hold apart the atoms of matter—would doubtless be sufficient to resolve every known substance into its simplest elementary constituents, perhaps into a very few primordial elements; for chemists are far from being satisfied that they have arrived at the ultimate forms of matter in their list of

sixty-five elements. But, however this may be, we know that the atoms, whatever they were, must have been held so far apart that no combinations could possibly have existed. Neither were they drawn more in one direction than another by their mutual attractions, for they are equally diffused through all space. Therefore, heat, the great repulsive force, has overcome all the forces of attraction—cohesion, chemical affinity, and gravity.

Between such mighty contending forces we can hardly imagine a state of perfect equilibrium. Immense currents and world-wide surgings must be the long-continued if not the permanent condition of this state of things, especially if we conceive it brought about by natural causes. More condensed portions of nebulous matter would be formed—sections of space larger or smaller, in which the forces of attraction counterbalanced those of repulsion. Each such section would then have its centre of gravity, around which all the currents within its influence, by the law of the composition of forces, must eventually unite in one. This one flowing ever around and slowly toward the centre, like a ball rolling down an inclined plane, goes faster and faster, until the centrifugal overbalances the centripetal force, and it separates completely from the inner mass. Thus a ring is formed revolving around a central nucleus. Unless perfectly equiposed, and of homogeneous material, this ring would sooner or later break up into a number of globes, which, by the superior attraction of the largest, would ultimately coalesce into one. This globe, still contracting, and the nucleus also contracting, would throw off satellites and other planets, all revolving in nearly the same plane and in the same direction. All these processes are in perfect accord, not only with the conditions of the heavenly bodies so far as discovered, but with known natural laws. Many of them have been successfully imitated on a small scale in experimental illustrations, as in the rapid rotation of oil suspended in water.

We have here given only the simple outlines of the famous "nebular hypothesis" of Laplace. In later years, the discovery of nebulæ in the heavens in all stages of world-formation, the evidence of the spectroscope on the unformed material of the universe, and other proofs, have compelled for the proscribed hypothesis a recognized place in science. We do not stop to consider these subjects more fully, because it is the purpose of this article to inquire chiefly concerning the forces that would be engaged in such a process of evolution; and, firstly, how from the preponderance of the repellent forces holding matter in universal diffusion there came the final mastery of the aggregating forces ever concentrating, combining, and working up the materials of the universe.

The first of the operations which have come to our notice in the progress of this evolution is the condensation of the gases. This, according to all experience, ought to evolve heat; but, instead, we

find only that the flow of the currents—the motion of the masses—is proportionately increased. Is there a connection of cause and effect between these phenomena?

All motion that we are familiar with requires the expenditure of heat. The combustion of coal supplies motion to the steam-engine. The evaporation of water by the sun's heat causes the rain-clouds and the mill-streams. The oxidation of certain elements in the food we eat is the combustion which supplies our bodies with powers of motion. Recent discoveries have shown not only that motion is heat transformed, but that to produce a certain quantity of motion an invariable certain quantity of heat is required.

Again, the cessation of motion evolves heat. It is well known that by skillful blows with the hammer a cold iron bar can be made red-hot. Two wheels revolving in opposite directions, and touching at the circumference, become highly heated; and factories have been warmed solely by this transfer of motion into heat. Friction is but another name for the arresting of motion, and, as we well know, always produces heat. There is also here the same equivalence as in the other case. The stoppage of motion evolves just the amount of heat that was required to produce that motion.

The greatest triumph of modern science is the splendid induction that all the forces are correlative and indestructible. Not an impulse of motion, of light or heat, or any force, is ever lost. It may be communicated from one body to another, or transmuted into some other form of force, or become for a time latent or imperceptible; but it always exists, and is reclaimable back again into the same, in mode and quantity, from which it started.

The grandest exemplification of these truths will be found in what we are now considering, the origin of the celestial revolutions. The condensation of gases gives out heat in direct proportion to the contraction of volume. The attraction of gravitation, not only between masses but between all the particles of matter, increases in the inverse square of the diminishing distance. From these two principles it can be mathematically shown that in the contraction of each great world-nebula heat would be set free in the precise proportion of the increase of atomic attraction; or, in other words, that it would take the exact amount of heat-force that had been released, to separate the atoms again to their original distance apart. But in this instance the heat-force is not really set free; it is transformed into the motion of the mass from which it came. Instead of holding the atoms apart, the work which it now has to do under the form of motion is, to prevent the masses from falling into each other. It is this motion—the celestial revolutions—which keeps the worlds apart, and allows each to work out its destiny under the aggregating forces, without interference from any other. Up to a certain point of condensation, which is previous to the radiation of heat into space, if this motion

were at any time stopped, it would be resolved into just the amount of heat necessary to expand the mass again to its original dimensions.

The attractive forces, gravity, chemical affinity, and cohesion, whether these forces are many or one, are inherent properties of matter. Every atom has its definite capacity of attraction, which may be exercised or not according to circumstances. For it is evident that an attracting body may be at the same time drawing toward itself a million other like bodies, or none at all, without change of its power of attraction. In like manner the magnet has a definite lifting power whether it is actually holding up a weight or not. If this attribute of matter is not operative, or but partially so, it is because heat, or motion, or some repellent force, is holding the atoms or the masses at a distance from each other, and thus opposing the exercise of it. The sum, however, of the attracting power belonging to the world of matter is as fixed as the quantity of matter itself. And I think it is in the highest degree probable that there is in the universe precisely enough repulsive force or heat to overcome all this inherent power of attraction. When all motion of the masses and of the atoms is resolved into repulsive energy, and brought to bear on the elements of matter, I imagine that they must completely fill the bounds or the infinity of space. Then, if there were perfect equilibrium or rest, no further changes or effects could ever be manifested. Such a condition, however, could probably never result from natural causes, for the time necessary to the perfect balance of the forces must be as infinite as the space through which they extend, and to "set bounds to space" has puzzled philosophy from a very ancient date. If, on the other hand, the universe of matter was created in a state of absolute rest, we have the further and necessary provision that the Spirit of God moved on the face of his creation, and thus unbalanced the forces. But the equilibrium once broken, in whatever manner, from that moment evolution must inevitably proceed. For, let there be an overbalancing of the aggregating force in ever so little or much, an equivalent of the opposing force must thereafter find some other work to do, and the field is effectually given up to the mighty agency that combines, and constructs, and brings order out of chaos.

So long and in proportion as the forming worlds continue to contract their dimensions, the rotations and revolutions increase in their velocity. Thus in the rapid and ever-speeding movements of the heavenly bodies there is stored up the ever-increasing reserve of heat that is liberated from the great contest with gravity. But in the progress of concentration there comes a time when the atoms of matter have approached each other sufficiently near for other forces of attraction, equally correlative of heat, to come into play—chemical affinity between molecules of unlike nature, and cohesion between those of like kind. Under the latter term are included all the changes of state which are the result of cooling. By these attractions heat is set free

in such abundance and under such conditions that it cannot be stored away in the motion of the masses. It is then, probably for the first time, that heat becomes a wave-force, and is radiated into space as light and radiant heat—not, however, lost, for that is impossible, but moving ever onward and outward to the day and the place of its final reclamation.

Our own solar system has already progressed far in this stage of aggregation. All the planets and satellites have become crusted over, and have ceased almost entirely to radiate heat. But the sun, the great central body, the one which should last of all become cold, is still in active combustion or chemical combination. Immense quantities of light and heat are still radiating from its surface—so immense that the little fraction which our earth catches as it flies through space gives us all the motion, and life, and beauty, which we enjoy. But the sun is not even now the glowing orb that once it was, as the rock-records of our globe testify. Its bright radiance is slowly but surely fading. Those huge, black incrustations, often twice as large as the whole surface of the earth, that float awhile on its photosphere, and then are suddenly broken up—they were not always there. And, if they have grown upon it, the uncomfortable conviction arises that they will continue to grow and darken more and more its life-giving face. Old age is certainly being written on the solar brow. It may be millions of years hence—for time is not one of the economies of Nature—but the period will surely come when light and heat will all have departed from the sun, as they once ceased to be radiated from the earth and the planets and the numerous stars that have gone out within the records of astronomy. A pall of darkness will gradually overspread the universe as one by one the stars of the firmament shall fade away and sink into gloomy, lifeless sleep. A day in the mighty calendar of creation has passed, and a night has followed, cold and dark as the tomb of expiring Nature.

But is there no awakening, no morrow to this night of the universe? Is the contest over, and never to be renewed? For answer, let us seek out in this case, as we did once before, the condition and movements of the great contending forces. Those of attraction have now in each world expended their utmost possible energy, and are holding all the forms of matter combined and compacted in a cold and rigid embrace. The forces of repulsion have entirely abandoned the contest, and are either vibrating through the unknown realms of space, or are locked up in the swift and complicated motions of the heavenly bodies. It is probable that by far the greater part of the repulsive forces thus exists in the form of motion. It has been estimated, no doubt with a near approximation to truth, that, if by any means the earth could be suddenly arrested in its rapid course, its mass would thereby be raised to the enormous temperature of 23,360° Fahr.—a heat sufficient to vaporize and dissipate every known sub-

stance. If then, as would be the case, it should fall into the sun, this heat would be increased by the fall four-hundred-fold. Now, it makes no difference in the aggregate evolution of heat whether this cessation of motion is sudden or gradual; and if we can find in Nature any agencies tending to retard the revolutions of the planetary bodies, they must inevitably sooner or later fall into the sun. In such a case it can hardly be doubted that we have found a cause sufficient to produce again the disintegration and diffusion of matter.

The wave-theory of light and radiant heat presupposes the existence of an ethereal medium pervading all space. It must be a medium of material atoms held in equipoise by a balance of forces, for it is evident there could be no wave-motion unless there was something to move, and something, too, having the attributes of matter in a state of extreme mobility or fluidity. There is no other conceivable way by which light could reach us from the sun and stars except through this all-pervading form of matter. And if there is a material medium, of whatsoever exceeding tenuity it may be, still it must present something of resistance to everything passing through it. It resists the passage of light eight minutes in 90,000,000 miles, thus proving its materiality by its resistance to force, which is one of the definitions of matter. If one could conceive of any force passing through an absolute vacuum, it could only be conceived of as passing instantaneously—there is absolutely nothing to detain it. Again, heat and its allied forces are only effects, and the subject is and can be only matter. There is no physical truth better established than that the forces can exist only where matter is in some form. It is not essential that this form of matter be subject to the ordinary laws of gravitation. The probability is, that it differs entirely from anything that we have experience of. It would seem that the atoms composing the ether of space, instead of attracting each other like those of ordinary matter, must repel each other. At least this supposition would account for what there is remarkable in connection with the ethereal medium. But, whatever theories we may adopt in regard to it, this is certainly true, that the revolutions of the heavenly bodies must be continually opening passages through it, and that a certain part of the force of those revolutions must be expended in pushing it aside. The centrifugal force is thus lessened, and the bodies are drawn nearer to the sun. In consequence, the periods of their revolutions are shortened. This has not as yet become noticeable in the case of the planets, from the fact that the slow contraction of their bulk by the loss of internal heat through volcanoes, thermal springs, and other sources, has the contrary effect of increasing the velocity of revolution, and thus counterbalancing the retardation of friction. The fact that the two effects are thus nearly counterbalanced proves the retardation, for otherwise we know that the acceleration would be observable. In the case, however, of the light cometary bodies, it has been shown that they suf-

fer a very considerable retardation in their passage through space. Encke's comet formerly came regularly back into the field of the earth's orbit once in every three years, but with a period shortened six hours each time. The whole planetary regions seem to be filled with collections of matter—star-dust and meteorites. They are all revolving about the sun in eccentric orbits, and are doubtless slowly circling toward it. The zodiacal light is supposed to be only an immense aggregation of this material. Thus the thickening stratum as these strange bodies draw near to the sun shows that they are all slowly gathering to that great centre of attraction.

The evident effect of the fall of any of the planets into the sun would be the diffusion of highly-heated vapors far out into the spaces that surround it—probably far enough to reach the next outlying planet, and thereby to increase its retardation and hasten its fall into the mighty caldron. So one by one the planets dissolve and their elements fill the void of space. The expanding gases catch up the waves of radiant heat that have long been wandering from planets and suns; and the nebula is again seething and surging with its mighty contending forces. Sun-system reaches out to sun-system, and star-galaxy mingles with star-galaxy, till through all the abysmal depths matter is again "without form and void, and darkness is upon the face of the deep." Chaos has returned once more, again to be breathed upon by the Omnipotent Spirit that reforms and recreates.

ACCOUTREMENT OF A FIELD-GEOLOGIST.

By Professor GEIKIE, F. R. S.,
DIRECTOR OF THE GEOLOGICAL SURVEY OF SCOTLAND.

FIELD-GEOLOGY does not mean and need not include the collecting of specimens. Consequently a formidable series of hammers and chisels, a capacious wallet with stores of wrapping-paper and pill-boxes, are not absolutely and always required. Rock-specimens and fossils are best collected after the field-geologist has made some progress with his examination of a district. He can then begin to see what rocks really deserve to be illustrated by specimens, and in what strata the search for fossils may be most advantageously conducted. He may have to do the collecting himself, or he may be able to employ a trained assistant, and direct him to the localities whence specimens are to be taken. But, in the first instance, his own efforts must be directed to the investigation of the geological structure of the region. The specimens required for his purpose in the early stages of his work do not involve much trouble. He can detach them and carry them off as he goes, while he leaves the full collection to be made afterward.

It is of paramount importance that the field-geologist should go to his work as lightly equipped as possible. His accoutrements should be sufficient for their purpose, and eminently portable. You may judge of the portability which may be secured when I tell you that I have on my person at this moment all the instruments necessary for carrying on a geological survey, even in the detailed manner adopted in the Geological Survey of this country. You observe, therefore, that a fully-equipped field-geologist need not betray his occupation by any visible implement. The want of such tokens of his craft often greatly perplexes rustic observers, to whom his movements are a fruitful source of speculation. I shall divest myself of my accoutrements one by one as I have occasion to refer to them, and describe their uses.

The *hammer* is the chief instrument of the field-geologist. He ought at first to use it constantly, and seldom trust himself to name a rock until he has broken a fragment from it and compared the fresh with the weathered surface. Most rocks yield so much to the action of the weather as to acquire a decomposed, crumbling crust, by which the true color, texture, and composition of the rock itself, may be entirely concealed. Two rocks, of which the outer crusts are similar, may differ greatly from each other in essential characters. Again, two rocks may assume a very different aspect externally, and yet may show an identity of composition on a freshly-fractured internal surface. The hammer, therefore, is required to detach this outer deceptive crust. If heavy enough to do this it is sufficient for your purpose; any additional weight is unnecessary and burdensome. A hammer, of which the head weighs one pound or a few ounces more is quite massive enough for all the ordinary requirements of the field-geologist. When he proceeds to collect specimens he needs a hammer of two or three pounds, or even more, in weight, and a small, light chipping-hammer to trim the specimens and reduce them in bulk without running a too frequent risk of shattering them to pieces.

Hardly any two geologists agree as to the best shape of hammer; much evidently depending upon the individual style in which each observer wields his tool. This (Fig. 1.) is the form which, after long experience, we have found in the Geological Survey to be on the whole the best. A hammer formed after this pattern combines, as you observe, the uses both of a hammer and a chisel. With the broad, heavy, or square end, you can break off a fragment large enough to show the internal grain of a rock. With the thin, wedge-shaped, or chisel-like end, you can split open shales, sandstones, schists, and other fissile rocks. This cutting or splitting edge should be at a right angle to the axis of the shaft. If placed upright or in the same line with the shaft, much of its efficiency is lost, especially in wedging off plates of shale or other fissile rocks.

A hammer shaped as I recommend serves at times for other than

purely geological purposes. On steep, grassy slopes, where the footing is precarious, and where there is no available hold for the hand, the wedge-like end of the hammer may be driven firmly into the turf, and the geologist may thereby let himself securely down or pull himself up.

FIG. 1.—GEOLOGICAL HAMMER, COMPASS-CASE, AND BELT.

The most generally convenient way of carrying the hammer is to have it in a leather sheath suspended from a waist-belt. The hammer hangs at the left side under the coat, the inside of which is kept from being cut or soiled by the protecting outer flap of the sheath. Some geologists prefer to carry the belt across the shoulders outside, and the hammer suspended at the back. Others provide themselves with strong canvas coat-pockets and carry the hammer there.

Even the most sharp-sighted observer is the better for the aid supplied to him by a good magnifying-glass. For field-work a pocket *lens* with two powers is usually sufficient. One glass should have a large field for showing the general texture of a rock, its component grains or crystals, and the manner of their arrangement; the other glass should be capable of making visible the fine striæ on a crystal, and the minute ornament on the surface of a fish-scale or other fossil organism. Applied to the weathered crust of a rock, the lens often enables the observer to detect indications of composition and texture which the fresh fracture of the rock does not reveal. It sometimes suffices to decide whether a puzzling fine-grained rock should be referred to the igneous or the aqueous series, and consequently how that rock is to be colored on the map.

Any ordinary pocket-*compass* will suffice for most of the requirements of the field-geologist. Should he need to take accurate bearings, however, a small portable azimuth compass will be found useful.

This is the instrument employed in the Geological Survey. It is carried in a leather case, or pocket, hung from the waist-belt on the side of the body, opposite to the hammer (Fig. 1). The directions of the dip and strike of rocks, the trend of dislocations and dikes, the line of boundaries, escarpments, and other geological features, are observed accurately, and noted on the spot at the time of observation, either on the map or in the note-book. A convenient instrument for light and rapid surveys, or reconnaissances, combines the compass and the next instrument I have to describe—the clinometer. I shall refer to it again.

The *clinometer*, or dip-measurer, is employed to find the angle at which strata are placed to the horizon—an important observation in the investigation of the geological structure of a country, and one having frequently a special economic value—as, for instance, when it points out the depth to which a well or mine must be sunk. Various patterns have been proposed and used for this instrument. Formerly a spirit-level was commonly employed. But, apart from the difficulty of rapid adjustment for the requirements of the field, the spirit-levels in the clinometers were apt to get broken. A much more portable and serviceable form of clinometer may be made by the geologist himself. It consists of two thin leaves of wood, each two inches broad and six inches long, neatly hinged together, so as to open out and form a foot-rule when required (Fig. 2). On the inside of one of these leaves a small brass pendulum is so fixed that when it swings freely and hangs vertically it forms an angle of 90° with the upper edge of the leaf to which

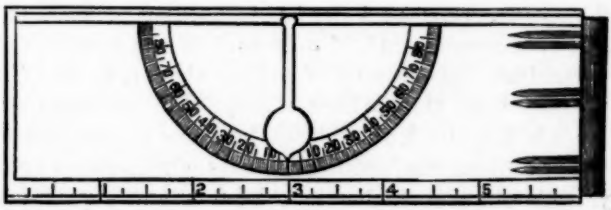


Fig. 2.—The Clinometer.

it is attached. An arc, graduated to 90° on each side of the vertical, is drawn on the wood, or on paper or brass fastened to the wood, so that when the leaf is moved on either side the exact number of degrees of inclination is shown by the pendulum on the graduated arc. The corresponding face of the opposite leaf is hollowed out just enough to let the two leaves fit closely, and keep the pendulum in its place when the instrument is not in use. This form of clinometer, made of boxwood and bound with brass, may be obtained of instrument-makers. It is light and strong, and its durability may be understood from the appearance of the instrument which I hold in my hand,

and which, though it has been in constant daily use for more than twenty years, is as true and serviceable as ever.

If at any time the geologist has occasion to lighten his equipment for some long mountain-expedition, where every additional ounce of weight begins to tell by the end of the day, and where, therefore, for the sake of doing as much and holding out as long as possible, he should carry nothing that is not absolutely needful for his purpose, he may advantageously combine the pocket-compass and clinometer in the one instrument to which I have already alluded. This convenient instrument is about the size of an ordinary gold watch. It consists of a thin, round, flat, metal case, shaped like that of a watch, and covered either with a common watch-glass, or, still better, with a flat disk of strong glass. Instead of figures for the hours and minutes, the white enameled face of this geological watch is that of a common pocket-compass. But the interval between each of the four cardinal points is divided into 90°. On the central pivot, just underneath the needle, a small brass pendulum is placed, and a straight-edge of metal is soldered on one side of the outer rim of the watch-case in such a position that the instrument will stand on it if need be, and the pendulum will then point to zero. A simple piece of mechanism passing through the handle enables the observer to throw the needle off the pivot, or let it down, as he may require.

As it is impossible for a field-geologist to remember the details of all the observations he makes on the ground, or to insert them on a map, he regards a good note-book as an essential part of his apparatus. From the nature of his work, he has frequently occasion to make rough sections, or diagrams, and, if possessed of the power of sketching, he has abundant opportunity of aiding the progress of his researches by jotting down the outlines of some cliff, mountain, or landscape. Hence, his note-book should not be a mere pocket memorandum-book. A convenient size, uniting the uses of a common note-book and a sketch-book, is seven inches long by four and a quarter inches broad. Let me remark, in passing, that perhaps no accomplishment will be found so useful by the field-geologist as a power of rapid and effective sketching from Nature. If he has this power in any degree, he ought sedulously to cultivate it. Even though he may never produce a picture, he can catch and store up in his note-book impressions and outlines which no mere descriptions could recall, and which may be of the highest value in his subsequent field-work. This is true of ordinary detailed surveys, and still more of rapid reconnaissances, which may have their ultimate usefulness enormously increased if the observer can seize with his pencil and carry away the forms of surface as well as the geological relations of the region through which his traverse lies.

As every device which saves labor and time in the field, or which adds to the clearness of the work, is deserving of attention, I would

refer here to the use of variously-colored *pencils* for expressing at once, upon map or note-book, the different rock-masses which may occur in a district. Water-colors are, of course, ultimately employed for representing the geological formations on the finished map. But a few bits of colored pencils carried in his pocket save the geologist much needless writing in the field. To a red dot or line he attaches a particular meaning, and he places it on his map without further explanation than the local peculiarities of the place may require.

This leads me to remark, also, that he necessarily adopts a system of signs and contractions on his map, not only to save writing, but to prevent the map from being so overcrowded with notes as to become hopelessly confused. Every field-geologist insensibly adopts contractions of his own. For the fundamental facts of geological structure, however, it is eminently desirable that the same signs and symbols should be used with the same meaning on all published geological maps. The subjoined diagram (Fig. 3) shows some of the signs used on the maps of the Geological Survey of Great Britain and Ireland.

Such are the few prime instruments required in field-geology. We may add others from time to time, according to the nature of the work, which in each region will naturally suggest the changes that may be most advantageously made. A small bottle of weak hydrochloric acid, carried in a protecting wooden box, or case, is sometimes of use in testing for carbonates, particularly in regions where rocks of different characters come to resemble each other on their weathered surfaces. When Sir William Logan was carrying on the survey of the Laurentian limestones of Canada, he received much help from what he called his "limestone spear." This was a sharp-pointed bit of iron fixed to the end of a pole or a walking-stick. He enlisted farmers and others in his operations, instructed them in the use of the spear, and obtained information which gave him a good general notion of the distribution of the limestone. The spear was thrust down through the soil until it struck the rock below. It was then pulled up, and the powder of stone adhering to the iron point was tested with acid. If, after trying a number of places all round, the observer uniformly obtained a brisk effervescence when the acid drop fell on the point of his spear, he inferred that the solid limestone existed below, and noted the fact on his map accordingly.

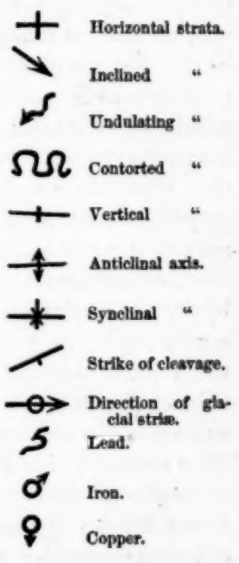


FIG. 3.—SOME USEFUL SIGNS IN GEOLOGICAL SURVEYING.

When the Geological Survey was busy with the great Wealden area of the southeast of England, my colleagues used what they nick-

named a "geological cheese-taster." It was, indeed, a kind of large cheese-taster, fixed to the end of a long stick. This implement was thrust down, and portions of the subsoil and of the clays or sands beneath were pulled up and examined. Similar devices must obviously suggest themselves according to the nature of the work in different districts and countries.

In the course of his observations in the field, the geologist will meet with rocks as to the true nature of which he may not be able to satisfy himself at the time. He should in such cases detach a fresh chip from some less weathered part of the mass and examine it further at home. The detailed methods of investigation, which may be pursued with all the conveniences of a laboratory in town, are not possible to him in the country. But he may subject his specimens to analysis in two cases, and obtain valuable, and perhaps sufficient, information as to their characters. He can easily fit up for himself a small and portable blowpipe-box, a machine for slicing and preparing rocks, minerals, and fossils, for examination under the microscope, and a microscope.

The *blowpipe-box* should contain a common blowpipe, platinum-tipped forceps, platinum wire, small bottles with the ordinary reagents, and as many of the most useful parts of blowpipe apparatus as the space will admit, consistently with the whole box being easily packed into a portmanteau. By means of the blowpipe, it is often possible to determine the nature of a doubtful rock or mineral, and to ascertain the proportion of metal in an ore. A young geologist should take with him to the field only the most essential apparatus and reagents; he will gradually come to see by practice what additions he may best make to his equipment.

A convenient and portable form of the *rock-slicing machine* is sold by Fuess, of Berlin. Where it cannot be obtained, the field-geologist may succeed in preparing his slices by chipping thin splinters from the rock and reducing them upon a grindstone or whetstone. One side of the splinter is to be made as smooth and free from scratches as possible, which can be effected by polishing on a water-of-Ayr stone. This polished side is then cemented with Canada balsam to a piece of plate-glass. When quite firm, the upper side of the stone is ground down until the requisite degree of transparency is obtained. Considerable practice may be required, and many preparations may be spoiled, before the observer becomes proficient. But the labor is well bestowed, for in no other way can he obtain the same insight into the internal texture and arrangement of the rocks with which he is dealing. He sees what are the component minerals of a rock, and how they are built up to form the mass in which they occur. He likewise can detect many of the changes which these minerals have undergone, and he thus obtains a clew to some of the metamorphic processes by which the rocks of the earth's crust have been altered.

The *microscope* should be, like the rest, as portable as possible. For most geological purposes high powers are not required, consequently a small microscope is sufficient.

It is sometimes of service, when working in a district where microscopic rock-sections are required, to carry a small collection of microscopic slices of selected or typical rocks or minerals for purposes of comparison. A series of fifty or one hundred slices can be packed in a box a few inches square.— *Outlines of Field-Geology.*

ON THE ANNIHILATION OF THE MIND.

By JOHN TROWBRIDGE,
ASSISTANT PROFESSOR OF PHYSICS, HARVARD COLLEGE.

THERE are some subjects which are unapproachable by any of the present methods of scientific investigation, yet the human mind, especially that form of it which is utterly untrained in scientific methods of thought, loves to ponder over the profoundest mysteries, and calls upon Science with an almost imperative tone to solve moral doubts and fears. One of the greatest questions which one finds is perplexing the general reader of popular science, who is also an independent thinker on religious questions, is that of the survival, so to speak, of the human mind and all that betokens the mental and moral power of man after death. The alarming doctrine that the mind and soul are the result of a process of growth in the individual, like physical growth of bone and muscle, and that body and mind increase and decrease together, and are resolved into the elements again at the close of life, is not infrequently put forward by materialists. It is maintained, further, that the belief in immortality is largely a matter of education, notwithstanding the evidence which is brought forward to prove that even uncivilized nations have a belief in deities and a future life. To the materialist, the picture presented by the unwrapping of a Peruvian family burial-sack, with its young and old mummies, and its collection of pottery and bag of grain to help the disembodied spirits on their way to a happier hunting-ground, is pathetic only because it seems a hopeless superstition. What kind of a soul, it is asked, has the Digger Indian who is hardly more intelligent than a wild animal? If he has a mind and soul, so has my dog. No; what we call the soul is a cultivated state or condition which perishes like a highly-disciplined adaptation of the muscles of the body which a gymnast possesses. It is a state of crystallization; it is a reaction or interaction of atoms consequent upon physical growth. When the body dies, the mind and its attributes perish. Such utter disbelief in the great doctrine of the resurrec-

tion is hard to combat; for, even among scientific thinkers, the class of men who do not become attached to the cast-iron ways down which thought has traveled to them is small. A logician who sets his mental machinery in motion, and then steps to one side to scrutinize its defects and limitations, is rare. To hint that there may be higher processes of logic than those generally accepted, implies the possession of a scientific mind, to say the least, not of a quantitative cast. It has seemed to the writer that a discussion of the idea of the degradation of spiritual energy, so to speak, would not be an unprofitable or irreverent subject from the purely scientific point of view. A little thought will convince one that no transformation of energy can take place in Nature without degradation or dissipation of it. In order to generate steam we must expend the energy stored up in the coal; and in its turn the steam in doing work passes from a hotter state to a colder one. A fresh supply of energy is needed in order to enable the cold body to do work again. There is a tendency to a uniform diffusion of heat, or to a degradation of energy.

In the process of physical growth and decay, the doctrine of the conservation of force, and the degradation of energy, is clearly exemplified. What the body receives from the sun in the process of growth is given back, transformed, to the earth. At death the physical being undergoes a chemical change; and the earth and air recall to themselves their respective portions. Here there is an equivalent rendering of matter. If the soul and mind have been the result of a process of growth, the entire potential energy of the living unit has not been accounted for in the final dissolution. The song of a bird can be resolved into waves of motion which, although they cease after a moment, and the consequent vibrations of the human ear die away, are still exerting an influence upon matter. Babbage, in his "Bridgewater Treatise," has drawn a powerful picture of the possible permanence of the motion which has been communicated to the ether by the tones of a human voice, and shows that it may not be impossible to believe that the eloquence of Demosthenes still continues in some form of motion. So we can believe that the physical effects of a bird's song can remain forever impressing some form of motion upon matter. Besides the physical vibrations which the song communicated to the human ear, it has so impressed the mind that, after the lapse of years, the repetition of the same notes can call up innumerable memories of deeds and a thousand pictures of the past. In the mind of the poet it may be the one detached note from which he can construct a song of home which can serve to arouse the ardor of the Christian Slav against the Turk, and store up a fearful potential energy which by its fall can destroy entire nations. Here we have, in the transformation of the vibrations of sound to another form of energy, a continual degradation of energy; but we may have by the same means an exaltation of spiritual potential energy which is unexplained

by our doctrine of the conservation of force, and seems to require the incoming of another element in our calculations. Where does appear the force of mind, the high courage, which can enable a feeble body to maintain a high potential energy out of the same physical materials which contribute to the formation of the sluggishness of others? It may be answered: What makes the difference between the energy of the blooded hunter and that of the dray-horse? Where does the difference appear in the final dissolution? With this latter question we immediately perceive the difference between the degradation of energy which accompanies that which recalls life, and that which is manifested in the combinations of matter. Gunpowder, fired by the concentrated rays of the sun, leaves only ashes and a rapidly-disappearing veil of smoke. It has impressed upon the ether vibrations which are forever undergoing rapid transformations. In regard to its physical nature it goes from inertness to inertness. A current of electricity is maintained by chemical action which takes place in a voltaic cell. As long as this action continues, the current can exercise its functions. When the potential energy of the chemical activity falls, the current dies away. From the earth the gunpowder can be reconstructed with exactly the same characteristics. From the earth beings endowed with life can be created by a process which is far beyond our ken, yet the new creations are never exact reproductions. We are forced to acknowledge that there must be something which is called the principle of life. If there is such a principle, does it die at the physical death of each individual? If so, we must modify the all-embracing scope of the doctrine of the conservation of force and its non-annihilation. When a body loses its heat, or its electrical charge, we can readily form the equation of transformation. With matter endowed with life we must join, by an additive or subtractive sign, an unknown function which we may term the life-function. In discussing such an equation of transformation of energy, we must refuse to admit such a term depending on the life-function, on the ground that we are dealing with matter and material forces, and that there is no energy distinct from that communicated by chemical processes. Or we must admit it; and make some assumption which can just as well be made in reference to its spiritual or non-physical nature as in regard to the peculiar relations which different organic compounds may maintain toward each other. The first step leaves an hiatus in our expression for the transformation of energy, and the second gives a choice of belief.

It may seem to some that the doctrine of Darwin is capable of being extended to intellectual philosophy; and, as certain animal types fail to flourish and perpetuate themselves because the conditions are not propitious, so we can admit the possibility that the South-Sea cannibal is endowed with a mind or soul germ which could be developed if the right conditions were at hand. In chemistry we

find many substances which are apparently identical in composition, but which possess diverse qualities. Certain conditions are requisite to produce different states of the same compound. If these conditions are not fulfilled, the required combination is not made. With the cannibal our equation of the conservation of force would require a small term to represent the mind and soul, but a comparatively large one, it may be, to account for that stress of the particles, so to speak, which manifests itself as life. The source of the physical energy is the sun's heat. Looking, therefore, at the problem of life and mind from a purely scientific point of view, we seem to require a source from which can come the principle of life, and which can create moral and intellectual growth in suitable soil and under fitting conditions. In the case of the energy derived from the sun's heat we have a cycle of operations in which there is no annihilation of force. If we grant that there is a source of life and mind independent of mere chemical change produced by the sun's heat, and if we adhere to the notion of the conservation of force applied to this principle of life and mind, we are led to adopt the idea of a cycle of operations in which there is no annihilation of spiritual force. The doctrine of the existence of the spirit after physical death seems to me not to be foreign to the scientific ideas of the conservation of force, which have now obtained such complete supremacy in the science of physics; or to the doctrines of Darwin, which are accepted by so large a body of eminent naturalists. Without the sun there would be an annihilation of force. When energy is dissipated, we find the sun exalting it again by processes which we cannot completely follow. The idea of a great source of life and mind, the prototype of our physical sun, which sets in motion a vast scheme for the survival of the fittest, and the exaltation of energy in vast cycles, is not inconsistent with the doctrine of the New Testament, and seems to be required in a philosophical theory which shall endeavor to account for the differences in that great spiritual world which are continually suggested to the human mind by the various types of mental growth.

THE FIRST "POPULAR SCIENTIFIC TREATISE."

By Professor S. P. LANGLEY,
of the Allegheny Observatory.

SOME one has said that there is nothing in all the world of commonplaces which was not once a novelty, and born from the conception of an original mind. The idea that science is not for the professional student only, but that every one will take an interest in its results if they are only put before the world in the right way—this notion which has now produced a literature of its own—even

this idea was once brand-new. At the present time, when the most recondite investigation is summarized and explained for the unscientific, so that what is capable of translation into common speech is discussed at tea-tables within a week after presentation, it is not easy to go back in imagination to a day when the student of Nature worked only for, and was judged only by, a narrow circle of his own, and most gentlemen and gentlewomen were not only completely ignorant of scientific thought and method, but would have felt in danger of acquiring pedantry in learning them. Such, however, was the state of things two hundred years ago in the then most cultivated society of Europe; and it was to Bernard le Bovier de Fontenelle that first presented itself the audaciously novel conception of writing a book which should render some of the results of science into a language comprehensible by the most fashionably ignorant, and in a style which should make science itself recognized as a permissible topic of discussion in the *salons*.

His happy thought was executed with a cleverness akin to genius: the book went into all languages, and is said to have been reprinted a hundred times during the last century. "Conversations on the Plurality of Worlds" was its title; and though it is by no means rare, and indeed remains a classic in its kind, it is probably nowadays known only by name to the majority of English readers. Yet, in its way, nothing better has been done since, or rather its way is one which has had no entirely successful imitator among all its numerous progeny. It will be interesting, then, to look at this original in a path since so well trodden, and in doing so it may be premised that the book appeared in 1686, and was addressed to such a circle of readers as then only French society and the court of Louis XIV. could furnish. The age of Corneille, Molière, and Racine, La Bruyère, La Rochefoucauld, and St.-Simon, Bossuet, Massillon, and Bourdaloue (and it might be added of Fontenelle himself), was certainly not devoid of literary culture, and yet that very culture had so completely excluded science that we shall presently see the marchioness, who is presented to us as a type of accomplished elegance, expressing complete astonishment at hearing that the earth turns round, and the most *naïve* wonder at the idea that her park and castle, and she herself, are actually turning too!

The "Conversations" are introduced with a description of a moonlight night in the park, where the author is walking with the marchioness, to whom he is paying his court, with the accompaniment of perpetual and somewhat insipid compliment, which seems to have been a part of the conversational dress of the time, and to have belonged to the fashion of the period as much as its lace-covered waistcoats.

The talk is first of the beauty of the night, and moves on in an easy and natural tone, till the author casually speaks of the stars they are contemplating as "these worlds." The lady asks for an explana-

tion, and, on being told that it is likely to prove too learned to amuse her, only insists the more on the perfect capacity of her sex for the reception of the most philosophic ideas, and demands a lesson on the stars at once.

"No!" replies Fontenelle, "never shall it be said of me that in a wood, at ten o'clock at night, I talked philosophy to the most charming person of my acquaintance. Seek your philosophers elsewhere!"

But it is vain for him to try to bring the conversation back to its former channel, and to represent how much better it would be to talk nonsense, "as any reasonable people would do in our place"—he has to yield; but the dialogue, often very lively, is represented through the book as carried on by the gentleman with the wish to pay his court under cover of talking science; while the lady is ever on the alert to call him back to his ostensible theme when she finds him trying to wander from it. We must perforce omit this in giving only a part, and that chiefly Fontenelle's; but even in teaching he will be found anything but dull. As his pupil is as ignorant as she is intelligent, he begins at the beginning:

"All philosophy, I said, is founded on two facts, that we have curious minds and poor eyes, for if your eyes were better you might see for yourself if the stars were suns lighting other worlds, or if, on the other hand, you felt less curiosity, you would not care to learn, which would come to the same thing; but everybody wants to know more than he can see, and there is the difficulty. If we could even see unmistakably what we see at all, that would be something gained, but we see quite wrongly, and so your true philosophers pass their lives in the unenviable condition of doubting what they do see, and trying to divine what they cannot. I always think of Nature as a great spectacle, something like the opera. From your opera-box you do not see the theatre quite as it really is, for the scenes and stage-apparatus are arranged for effect at a distance, and they keep the weights and wheels which put all in motion out of your sight. Naturally, you do not pay much attention to the principle on which all this works. But then, again, there may be a machinist down by the orchestra, who is puzzled by some stage-flight, which is unaccountable to him, and who feels that he must find out how it was done.

"The machinist, you observe, is something like the philosophers; but what makes the difficulty worse for *them* is, that in Nature's machines the cords are all hidden—hidden so neatly that people were a long time conjecturing as to what caused the movements of the universe. Just imagine, for instance, Pythagoras, and your Platos and Aristotles, at the opera—they and all their kind whose names are in such reputation. Suppose that they saw the representation of Phaethon borne off by the winds, that they could not discover the cords, and did not know what lay behind the scenes. One of them" (the author is here giving us samples of the philosophy still current

in his time) "says, '*Phaethon is carried up by a hidden principle;*' another, '*Phaethon is composed of certain numbers which make him rise;*' another, '*Phaethon has a certain attraction toward the top of the theatre;*' and a hundred such vagaries, which I should have supposed would have cost antiquity all its credit. Finally, Descartes" (an Englishman would have said Bacon), "and some other moderns, have said, '*Phaethon rises because he is drawn up by cords, and because a heavier weight descends.*' So now we have come to believe that, if a body move, it is because it is pushed or pulled, and one who could see Nature as it is would simply be seeing what is behind the scenes at the opera."

After this, Fontenelle goes on to sketch the history of his science, and thence to give an account of the Ptolemaic and other systems, which preceded the Copernican. Here, again, a happy image reminds us of a danger all system-makers share, as common partners in a weakness which is as universal as humanity:

"Before I explain the first of these systems, I beg you to remember that we—all of us—are like a certain madman at Athens you may have heard of, who took it into his head that all the ships which came into the harbor belonged to *him*. Our common failing is to believe all Nature created for our own use, and when you ask our philosophers what end is served by that host of stars, they will calmly tell you, It is there for us to look at.' In this way they could not fail to suppose that the earth was fixed in the centre of the universe, and that all the heavenly bodies were set to revolve about her, and give her light; the same propensity which leads one to desire the most honorable seat at a ceremony makes the philosopher in his system put himself at the centre of the universe if he can."

It will be seen, as Sainte-Beuve remarks, that Fontenelle possesses the art of scientific *insinuation* in the highest degree; in addressing his marchioness, he is here appealing to the intelligence of every ignorant person who, rather than resemble the Athenian madman, is cajoled into truth, and disposed in advance to reject Ptolemy's system, in favor of the Copernican.

The account of the Copernican system involves the (to the marchioness) entirely novel idea of the earth's rotation. This is presented to us in a lively picture of the scene which would be offered to a spectator suspended above the surface as the speaker imagines himself to be: "'Passing under my eyes I see all sorts of faces, white, black, and brown. First come hats, and then turbans, and then shaven crowns; now towns with church-spires, now cities with slender, crescent-tipped minarets, now porcelain towers, and then again wide oceans and dreadful deserts.' 'What,' she cries, 'then in the place where we are—I don't mean this park, but this very place in the air—there are people continually passing by, who come where we are now, and at the end of twenty-four hours we get back again our-

selves!' 'Copernicus,' I replied, 'could not understand it better.'" This novel theme continues to occupy them during their return to the house, and the first evening secures her belief for the new system.

The next morning, on Fontenelle's sending to ask how the lady has passed the night, and to politely inquire whether she has been able to sleep while turning, he is assured that she has already got used to the motion, and was able to rest as soundly as Copernicus himself could have done. With so apt a scholar, progress is rapid, and, by evening, we find them discussing the habitability of the moon, and the cause of the sun's light and heat. What is the view of our author (the subsequent secretary of the Académie des Sciences, and an authority in his day) on the source of supply for this immense expenditure of the solar energy? What theory does he adopt—how was it accounted for in his time? Listen to the explanation of the man who has just satirized so happily the fallacies of the schoolmen. It shines because "it is self-luminous in its nature." And this is given in good faith by Fontenelle as a *reason!*

Clever as he is, he is here in the bondage of his age; but he might yet have taken a lesson from a contemporary, who, though pretending to no "philosophy," had seen and laughed at the weakness of the learned of his time in thus making words do duty for facts. We remember how the candidate for medical honors in the "Malade Imaginaire," on being asked why opium induces sleep, replies to the delighted satisfaction of the examining Faculty that it is because it possesses a soporific quality! When we see a man so acute as Fontenelle giving a precisely similar answer, with an obtuseness so plain to us, so imperceptible to him, can anything suggest more pertinently the need of watchfulness for traces of this legacy of ancient fallacies of thinking in our own modes of thought?

The third evening is occupied with a further discussion of the moon, and of Venus; on the fourth the other planets are considered, and reasons given for their possible habitability, some of which would hardly satisfy a more modern philosopher. Thus, the ingenious but scarcely satisfactory suggestion is made that, in spite of the neighborhood of Mercury to the sun, that planet may be a comfortable residence, owing to the presence there of large quantities of saltpetre, a substance which (according to our author) gives out "cold exhalations" in the sunshine. Lest this idea be unacceptable to our skeptical age, it should be added that Fontenelle takes care to fortify his position by citing the case of China, large portions of which, it appears, in spite of a southern latitude, experience extreme cold, even to the freezing up of their rivers in July, on account of the existence of this ingredient in their soil!

The "vortices" of Descartes are here introduced and offered as an explanation of the motions of the Jovian satellites about their primaries, and of the principal planets about the sun; and, in the next

evening, are applied to elucidate the constitution of the milky-way, in which worlds are, it seems, so thick that the plausible suggestion is made that their birds may fly from one to the other!

The remainder of the work is chiefly occupied with a description of the heavenly bodies considered with reference to their possible habitants, and here Fontenelle is not likely to be found tripping, for as to the nature, ways, and modes of living, of the inhabitants of the other planets, he is quite as well informed as we are. We shall find here nearly all that can be said, in the simple absence of any knowledge whatever on the point in question, but we may be more reasonably interested in the happiness of some conjectures offered, where he incidentally speaks of the physical constitution of the bodies he is considering. He tells us, for instance, that the rings of Saturn are supposed to be composed of numberless little moons, close together, and moving in the same orbit; an explanation which appears to have been lost sight of till modern analysis showed that they could not be continuous solids, and modern observation that they could hardly be liquid or gaseous. We have passed over too readily, perhaps, the purely speculative portion of the work, which, if not very instructive, is certainly entertaining, and filled with felicitous illustrations, such as that (too long for quotation) of the citizen of Paris, who maintains that St.-Denis, whose houses he can just distinguish from the towers of Notre-Dame, is uninhabited, because he can see no inhabitants. Or, for still another instance of this art of "scientific insinuation" already referred to, take the passage where the marchioness, after declaring herself dissatisfied with extravagant speculations about the inhabitants of the planets, is told that something positive is, after all, really known about a race on one of them, and which appears from his description to be remarkable indeed. He gives a minute, and, as he asserts, a trustworthy, account of these extraordinary beings, who he would have us believe are most laborious and skillful, yet live by pillage; who have no sex, yet increase as a nation; who subsist in the happiest concord, yet periodically put to death a portion of their innocent fellow-citizens; and so on, until the lady, who finds the story more incredible than any of the preceding speculations, on learning what the race is, and on what planet they exist, is forced to admit that truth may be stranger than fiction, and that no extravagance of his fancies about the possible commonwealths of other worlds surpasses what she has just been entrapped into listening to about that of the *bees* on our own.

Fontenelle, with all his abundant ingenuity, has one radical defect as a literary artist, and perhaps some will be disposed to add, as a student of Nature. He appears to have no power of moving or being moved by anything like emotion, or of perceiving anything not comprehensible to an intellect divorced from sympathetic intuition. The gallantry which he introduces as an element in the dialogue, and

which our citations do not undertake to illustrate, is filled, for instance, with ingenious conceits which, though falling coldly on modern ears, were considered in the happiest taste by the audience to whom the book was addressed. But they are of an artificial cleverness, and precisely what we might expect from the man who was said to have "as good a heart as could be made of brains."

Sainte-Beuve has well indicated our author's strength and weakness by comparing his clever opera-box view of Nature with that of Pascal in the majestic movement of the awe-inspired passage at the beginning of the "Pensées."[1] While agreeing to the judgment of the great critic, it may be observed, however, that, if Fontenelle be devoid of poetry, he has at least one image of a grace nearly allied to it. He has been speaking of the chances of the sun's light failing us wholly, as it is said to have partially done in the year following the death of Cæsar, and pointing out, with what seems justice, the imperfect grounds for the confidence of mankind in the constancy of Nature's action here in the future as in the past, founded as that confidence is on an experience of the human race—so long, judged by *its* life, so short in comparison with Nature's own. With a sort of pathetic sense of the fallacy, he compares this little accumulated experience of the generations of man to the traditions of some roses of a day, leaving each to its successor an account of the gardener in whom successive ages of these ephemeral flowers have seen no change. "*We have always seen the same gardener; in the memory of roses none has been seen but him; what he has ever been, that he is now: surely he does not die as we, or change.*"

Fontenelle, throughout the "Conversations," adopts the Cartesian hypothesis of "vortices" in accounting for the planetary movements, and this, indeed, he continued to cling to long after. The true theory of gravitation had been given by Newton, and obtained complete acceptance in England. His more serious work is to be found chiefly in the well-known "Éloges," which, as perpetual secretary of the French Academy of Sciences, he pronounced on its deceased associates; but it is probable that he will, nevertheless, be remembered as much by these "Conversations," which, had they no other merit, would always possess an historical interest, as opening the way to our present popular scientific literature.

We should not close this imperfect notice of them without again reminding the reader that the form of a dialogue gives the original an attraction which is necessarily missed in brief extracts, and that the plan on which they thus rest for uniting instruction and amusement (a plan which obliges the imaginary speaker to be paying his court and talking science at the same time) would have been a failure in almost any hands but those which could manage so difficult a blending, and keep as far from pedantry as from ridicule. Fontenelle's

[1] "Que l'homme contemple donc la nature entière dans sa haute et pleine majesté," etc.

gallantry, like his science, may be now a little out of date, but he manages at least to unite these two most opposite conversational ingredients in chemical union. When the lady would send him back from love-making to astronomy, he contrives to give both together, and, consistent to the close, takes leave of his charming scholar with the modest request that, as sole reward for his pains in teaching her the heavens, she will never again look on sun, moon, or stars, without thinking of *him*.

THE BALL-PARADOX.

By THOMAS S. CRANE, Mechanical Engineer.

THE exhibitors of the atmospheric air-brake, at the Centennial, attached a tube to the air-reservoir for the purpose of showing the immense pressure employed.

The current rushing from the small orifice of the tube sustains balls of varying gravities, according to the pressure applied.

Once, on accidentally resting the base from which the tube springs upon something lying on the table, it was found that, although no longer vertical, the current of air still held the ball in suspension, the ball revolving rapidly, and apparently hanging to the jet of air, which strikes the sphere at its upper side.

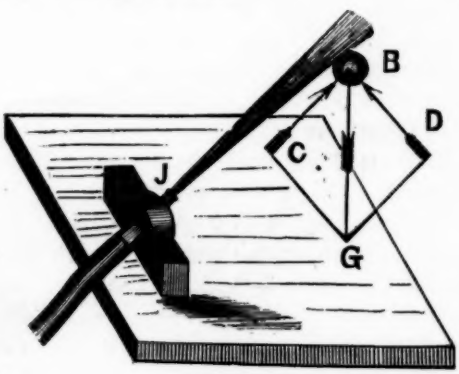


Fig. 1.

It also makes little difference in the result whether the ball be a solid glass one an inch and a half in diameter, or a hollow rubber ball, or a solid wooden one three or four inches in diameter, the only variation being the distance at which the spherical body is held from the orifice.

When a glass ball with interior colored lines, such as children play with, is gently held in the current until the air has the sphere well in its power, it will rotate partly back and forth at first, and,

when really revolved by the force of the air, has an uncertain axis of rotation until it has been turning for some time. The jet of air will also sustain a larger ball of lighter gravity behind the glass one, the former hanging on the lower side of the jet behind the heavier and smaller one.

These phenomena have excited so much attention that the following is offered as an illustration of the principles involved, and in explanation of the various points noted above:

The current of air sustains the ball by removing the atmospheric pressure from the ball where it strikes it; the unbalanced pressure of the air on the opposite side then forcing the ball toward the current, as shown by the arrow marked *D* in Fig. 1. The friction of the jet *J* against the ball tends to throw it in the direction indicated by *C*. To balance these two forces, we have the action of gravity, shown at *G*, which, being a constant factor, must be exactly neutralized by the forces named for the ball to remain suspended.

This adjustment, nice as it is, can be easily effected by placing the ball near enough to the jet at first; for, the pressure *D* being ample to sustain the ball in any position (else the experiment cannot be performed), the force of the jet will inevitably drive the ball away to a point where the power in that direction is just able to balance the force of gravity—the ball being evidently lifted, in spite of gravity, so long as it moves in the direction of the jet, and the force *C* naturally diminishing as the distance from the orifice *J* increases.

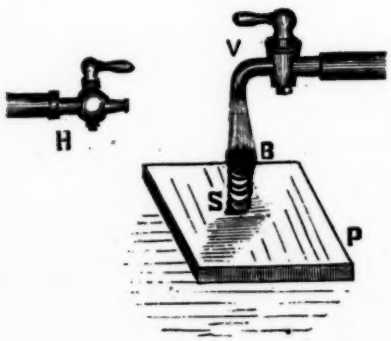


FIG. 2.

To make it clear that it is the ordinary atmospheric pressure that supports the ball, let us imagine it exposed to two forces (as shown in Fig. 2), acting on opposite sides of the sphere. The atmospheric pressures that act on all other sides of the sphere are ignored, as they perfectly balance one another.

If a ball *B* (Fig. 2) is secured to the top of a spiral spring *S*, attached to a platform *P*, and a jet of water *V* projected vertically upon it, the spring will be compressed until its resistance is equal to

the force of the jet. In that position the ball resembles a sphere exposed to atmospheric pressure above and below.

Now, let an horizontal jet of water *H* be thrown against the vertical jet *V*, as shown in Fig. 3, and its action, opposed to the spring *S*, must cease at once, and the unbalanced pressure of the spring raise the ball into the position shown in Fig. 3.

The situation of the ball in Fig. 3 is now equivalent to that in Fig. 1, where the air-jet *J*, passing over one side of the ball, calls into action the pressure *D* at right angles to the jet. Were the sphere *B* in Fig. 1 placed in the centre of the jet, no such action could result; and, if the ball were placed there, its gravity would be unbalanced until the ball fell to one side of the jet, and the supporting power of the air evoked.

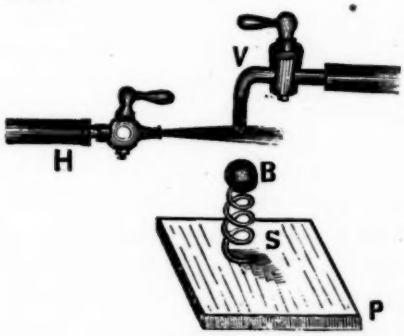


FIG. 3.

To prove that a current of air moving parallel with any surface destroys the atmospheric pressure at that point, take a visiting-card and bend the ends at right angles with the card, so as to turn up one quarter of an inch at each end. Now place the card near the edge of a smooth table, supported by the two ends like a little bench, and try the effect of blowing violently between the card and the table in a direction parallel to both. The current will destroy the atmospheric pressure beneath the card, and the unbalanced pressure above will force the middle of the card downward. Or, take another card and fit a quill or straw tightly into a hole cut in the centre. Try to displace another card laid loosely over the first by blowing upward against it. If a pin is stuck through the centre of the second card, into the opening of the quill, to keep it from sliding off, it will be found that no effort will blow the upper card from the first, as the current of air passing out between the cards destroys the atmospheric pressure between them, calling into play a force upon their outside surfaces that presses them tightly together. The arrangement of the cards is shown in Fig. 4.

It is thus evident that a pressure perpendicular to any surface can be displaced by one acting parallel to it.

An experiment illustrated in Fig. 5, and successfully performed while writing these lines, will settle all doubts, if any remain, upon this puzzling point.

Attach a pith-ball to a short piece of thread, and, knotting the other end, slip the knot in a slit in one end of a quill so as to secure it firmly, and retain the ball about an inch from the other end of the

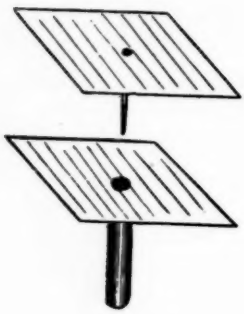


Fig. 4.

quill, as shown in Fig. 5. Now blow through the quill *Q* steadily, and the ball *B* can be made to hang from or upon the under side of the jet *J*, being prevented by the string *S* from blowing away, and the atmospheric pressure *A* balancing the gravity *G* of the light ball.

As the atmosphere presses nearly fifteen pounds upon a square inch, a ball one-quarter of a pound in weight would be balanced by the full pressure of the air upon a surface only $\frac{1}{60}$ of an inch in area. It remains to notice the rapid revolutions of the ball and its uncertain axis of rotation when first suspended in the jet.

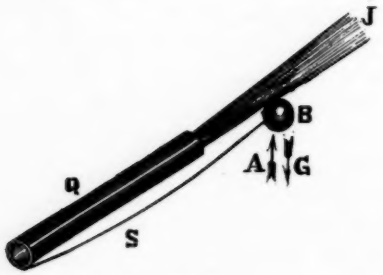


Fig. 5.

Few balls being perfectly round, or of uniform density, will revolve on their own centres under such conditions, but on the shortest axis passing through their centre of gravity (seldom the centre of form).

It is easy to show that all bodies freely suspended tend to revolve on their shortest axis, by tying a string a yard long to a door-key, just under the head, so that it will hang nearly vertical. If the string

be now rapidly twisted by the thumbs and forefingers of both hands, the key will assume an horizontal position, and the string revolve in the form of a cone.

The friction of the jet must, therefore, tend to rotate a ball on an axis at right angles to the path of the jet, and, if this axis is not the shortest one passing through its centre of gravity, several oscillations, back and forth, must occur before the necessary adjustment is made.

When two balls of different densities are sustained by the same jet, it seems plain that each is sustained by the pressure of the air on the side opposite to the contact of the jet, for it is evident that, farther from the orifice, the jet has less power to displace the atmospheric pressure, and at that point the lighter ball only can be sustained.

In its rapid revolutions in such a jet of air as we have described, a light and hollow India-rubber ball affords a beautiful illustration of the flattening of the earth's poles by its revolutions on a free axis.

LABORATORY ENDOWMENT.

By F. W. CLARKE,

PROFESSOR OF PHYSICS AND CHEMISTRY IN THE UNIVERSITY OF CINCINNATI.

THE advancement of science is at once a glory and a disgrace to our modern civilization. It is glorious that so much has been done, but disgraceful that the public should be so often indifferent to the doing. In view of the benefits derived from scientific research, it would seem as if governments and communities ought to vie with one another in its encouragement. But, as a matter of fact, this assistance has in every country been unsystematic, meagre, partial, and infrequent. A museum may be equipped, perhaps, an exploring expedition fitted out, a geological survey established, or a party of astronomers sent forth to observe an eclipse or transit. Even these things are too often done grudgingly, and on a basis of false economy. Physics and chemistry, the two sciences most immediately bound up with modern progress, have received little or no public aid. No laboratory exclusively for research has yet been endowed either by national or private enterprise. Colleges enough have been founded, with laboratories more or less fitted for the work of routine instruction; but these are manifestly unsuited to the production of remarkably far-reaching results. Every great industry in America has been directly benefited either by one or the other of the two sciences in question; fortunes have been made from practical applications of their principles, and yet scarcely anything has been done for them in return. It would seem as if our manufacturers expected to get applications of science without any science to apply. Nearly all

the research accomplished, either in our own country or in Europe, has been done by university or college professors, in the intervals between their regular duties, as an incidental matter, and usually with meagre appliances. Two results have followed: first, much wasting of individual energy; and, secondly, a lack of coherence in the knowledge won. The data of science become unsystematic, scattered, full of gaps and breaks, more like an archipelago than a continent. A thousand investigators, working independently and with but casual reference to each other, may discover a vast number of important facts, advance many useful arts, and yet accomplish but little for definite, exact, systematic, coherent science. The world gains much by their labors, but only a tithe of what it might gain were those labors wisely aided and fostered. For the present state of affairs, however, nobody is to blame. It is probably an unavoidable incident of scientific growth. A wider public culture and a deeper public appreciation will undoubtedly correct it. Looking forward hopefully, then, we may ask how the greatest good is to be done.

That the two sciences already mentioned are much in need of material encouragement, there can be but little doubt. They are experimental sciences, requiring for their advancement expensive apparatus and materials, such as individual students cannot provide for themselves, or few universities supply. Other branches of knowledge have been better provided for. Every observatory is to a certain degree a laboratory for astronomical research; every well-arranged museum affords opportunities for the scientific naturalist; every geological survey is ostensibly an organization of investigators. But a college laboratory, full of elementary students, each calling for personal attention from the professor, can hardly supply the best means for really advanced work. To be sure, every professor ought to do something, if only to discover a single small fact a year. Even though that fact be a hopeless negative, it will still have a true scientific value. When we question Nature, every answer, whether yes or no, counts for something in the upbuilding of science. Every man who is fit to teach science at all is competent to do at least a little in this direction. A little also may be accomplished by students; such work, for example, as the determination of densities, or completing the description of simple compounds. In one laboratory special attention might be paid each year to a single class of not over-complicated substances; and the advanced students could for that class fill up some of the gaps in our knowledge. But this work, although of immense value to science, is not of the very highest order. The most important labors can scarcely be undertaken save in laboratories specially and liberally endowed for purely scientific research.

The objections which are frequently urged against the endowment

of research arise from a misconception of what is really intended. It is ordinarily assumed that such endowments would merely provide large salaries and abundant leisure for certain scientific men, who, with no clearly-defined duties, and no distinct relation to each other, should try experiments at their own sweet will, and make discoveries whenever luck and chance were favorable. Such a vague plan for aiding science would of course be objectionable. Not only might the so-called "young men of promise" become deprived of energy by the ease of such positions, but even experienced workers would be liable to regard their salaries as the means of comfort without hard labor, or as a reward for past achievements. The money thus expended might advance science a little, but probably not so much as if it were paid over to some first-rate college or scientific school. Science is not to be truly encouraged by the creation of mere sinecures for scientific men. To construct an argument, however, against such a plan as the one mentioned, would only be to demolish a very clumsy man of very coarse straw. A true laboratory for research is something quite different from the feather-bed institution commonly objected to.

That there should be facilities for research, and that the investigator deserves a livelihood, nobody will deny. Indeed, these two points form an almost conclusive argument in favor of the endowment of laboratories. There is yet another consideration of very great force with which the public mind is less familiar. Both in chemistry and in physics there are many unsolved problems too great for individual students to grapple. Their solution can be effected only by the coöperation of many trained specialists, working harmoniously together upon the basis of some definite plan. The fundamental principles of physical science, principles upon which rest many applications important in the arts, and in which every manufacturer has a direct although too frequently unconscious interest, are to be eventually based upon the answers to these problems. In many a branch of industry thousands of dollars have been spent upon scientific experiments, which, for want of fundamental principles, have been aimless and unsystematic. Mere tentative trials, costly and laborious, with almost even chances for and against success, have taken the place of rigorous, careful, strictly scientific work, based upon definite and certain foundations. In short, the arts have suffered from the fact that neither chemistry nor physics can yet claim to be really an exact science. The question of the endowment of research, then, may well be put in this shape. Laboratories should be established in which adequate corps of thorough specialists shall coöperate in those investigations which individuals cannot undertake.

In a laboratory organized upon this idea, every worker should be assigned to definite, positive duties, the accurate and careful performance of which would eventually be sure to advance exact knowledge.

Here there is nothing speculative or doubtful. Here are certain things to be done, which can be done only by men of the most thorough training, equipped with the best appliances. In such a laboratory, chance would have but little place. The work would run to hard routine, to the solid establishment of accurate scientific data, to the systematic determination of substantial facts. Precise physical measurements would precede generalization, just as the labor of the quarryman goes before that of the builder. Startling and brilliant discoveries might possibly be made, but incidentally rather than as the result of special effort. The real value of the institution would be independent of anything sensational, and would rest upon considerations of the most severely practical kind.

Examples of the sort of work appropriate to an endowed laboratory may easily be found. For instance, one of the greatest of all scientific problems is that of the connection between the composition of a substance and its physical properties. Suppose this question were to be taken up systematically by a well-organized body of investigators. The first step in the research would manifestly be to determine, carefully and with the utmost rigor, the physical properties of the so-called chemical elements. At the outset each one of these substances would have to be isolated in quantity, and in a chemically pure condition—a labor which of itself would involve a great amount of research. Some of the elements have never yet been seen in a state of absolute purity, or have been obtained only in very minute portions, and accordingly new methods of treatment would need to be devised. Then would come the measurement of physical relations, thermal, electrical, optical, magnetic, mechanical, and so on. For each element, as far as possible, should be determined the melting-point, the boiling-point, the density, the coefficient of expansion, the specific and latent heat, thermal and electrical conductivity, the thermo-chemical constants, and many other data of much importance and value. Furthermore, these constants should be determined under widely-varied conditions, notably of pressure and temperature. For example, it would be necessary to ascertain the coefficient of expansion, and also the specific heat of a body at every degree, through a wide range of temperatures, and in not merely one, but in several series of observations. Thus, and thus only, could we attain to the exactness which science rigidly demands. Besides the actual measurements, this great labor would in many cases involve the comparative testing of various methods of research, and in some instances the invention of new experimental processes.

Years could be spent upon the metals alone, and the work done would add not only to our knowledge of their properties, but also much to science as regards variety and precision of methods. In connection with these researches would naturally arise an investigation of metallic alloys—a subject of which true science knows as yet

very little. The material accumulated would undoubtedly so systematize and extend our knowledge of these important substances, that we should soon be able to determine in advance all the properties of a proposed alloy, and even to ascertain by calculation what alloys could or could not be formed. The extent of research here suggested may be realized when we remember that, out of the sixty-five elements now known, not one has been thoroughly described, or described with even a moderate approximation to thoroughness. These investigations upon the elements would be for chemistry and physics what the preparation of star-maps and planetary tables is for astronomy, or the dissection of the human frame for medicine. They would certainly furnish a foundation for exact physical science such at at present is scarcely even begun.

After the examination of the elements would come the consideration of compounds. These should be taken up, series by series, in some regular order, and at least every typical body carefully described. Thus, step by step, the lines of assault would be drawn around the besieged problems, until at length the citadel would yield, exactness would replace the present chaos, and definite laws would stand where now are speculations. Could any branch of applied science fail to reap a benefit from this result? Would not every industry in any way dependent upon either chemistry or physics be helped? Apart from direct applications of science to the arts, the mere substitution of accuracy for inaccuracy in questions of scientific principle ought greatly to facilitate technological investigations, put new weapons into the hands of the artisan, and so add immensely to the resources of civilized life.

The investigations here indicated are not by any means the only researches proper for an endowed laboratory. They are merely types, to illustrate the general character of work which such an institution should do. It is true that, although individuals cannot deal with these greater problems in their entirety, individuals may, working separately and disconnectedly, contribute much toward their solution. But, unfortunately, researches of this kind are among the most difficult and arduous. They savor much of hard routine and yield no quick return of glory to the investigator, who, already familiar with monotony in his ordinary duties, naturally prefers to undertake labors producing with less effort a more immediate reputation. The discovery of new compounds is less troublesome, and brings speedier celebrity; hence the more solid work of establishing accurate numerical data is very little done. When done, it is done piecemeal. Garden flowers are so much easier to raise than oaks.

Now, assuming that a laboratory for research ought to be established, let us consider some of the leading questions as to its arrangement and organization. First, with regard to the building. This need not be very expensive, since architectural experiments have no necessary

connection with the purposes in view. It should be plain and substantial outwardly, sufficiently spacious within, accessible to much sunlight, and away from the heavier jar of traffic. The suburbs of a great city would be perhaps the most advantageous position for it to occupy. The most serious considerations, however, would concern its interior arrangements. It should, of course, contain a sufficient number of rooms for the accommodation of different branches of research; for example, a photometric room, another for gas analysis, a third for electric measurement, a fourth for calorimetric work, and so on. In the basement, connecting with the solid earth, might be placed a number of heavy stone piers for the support of very delicate instruments. One important item of apparatus would be a steam-engine. This, together with the chemical furnaces and a small machine-shop, might be provided for in a cheap out-building apart from the main structure. Steam, gas, and water, should of course be available in all parts of the laboratory.

But although expense could be avoided in the building, it ought not to be dodged in the purchase of instruments. These would necessarily be of the most costly character. Mere models, such as are commonly used for class instruction, would not suffice. Every instrument of precision used in the laboratory should be a standard of its kind, the best which could be made; otherwise the work of the institution might fall short of the high character intended for it. So also with the chemicals: only the best should be tolerated. As for a library, fitted for scientific reference-work, the cost would depend much upon locality. In a country town, away from other collections of books, the expense would be considerable; but near a city provided with libraries the outlay need not be very great. Still, some money would have to be expended in this very important direction.

Next as regards the working-staff. Since the researches to be undertaken are mainly those which involve the coöperation of specialists, we must start with a sufficiently large and varied body of men. At the head of the institution there ought to be a man of thorough training, proved ability, broad general ideas, and great executive capacity. He should guide and systematize all the work of the laboratory, and to him, as to the director of an observatory, the others should be subordinate. Under him should be at least the following corps of principals: one chemist, one electrician, a specialist in heat, another in optics, a mathematician, and an expert mechanic. Upon the last-named individual would devolve the duty of constructing, altering, or repairing apparatus. To this main staff might be added assistants, as many as the means of the laboratory would allow. Some of these minor positions could perhaps be filled by means of fellowships, analogous to those recently established by the Johns Hopkins University. It might be feasible also to admit private investigators and post-graduate students to the advantages for research afforded

by the laboratory, with the understanding that, in return for favors received, they should contribute a certain amount of labor toward the main purposes of the institution. Such volunteers, if I may call them so, could give, say, one-third of their time to this general work, and have the remaining two-thirds for their own investigations. Thus the laboratory might often aid young men of promise and ability, and derive real benefit from them in return. This power of encouraging and directing the beginner in research would not be least among the merits of the institution. For want of just such encouragement many and many a young enthusiast is driven out of scientific work into some field of labor less congenial and often less important. How much the world has lost in this way, how much science has been retarded, no one can ever estimate.

But how much money is needed for all this? That depends partly upon locality, partly upon other circumstances. In a place where building is cheap, real estate low, and living inexpensive, a moderate endowment would go much farther than in a city like Boston or New York. Under the most favorable conditions perhaps half a million dollars would suffice. Such a sum is by no means extravagant. Single individuals have given us much and sometimes a great deal more toward the establishment of a college, school, art-gallery, observatory, library, or hospital. Why not, then, half a million for a laboratory, three-tenths or less to go for building and equipment, and the remainder for permanently endowing the institution? Even a million would not be too much by any means. There are in our country a good many men able to give as much as this, whose fortunes have been made from applications of science to the arts. Here, then, is a chance for them to reciprocate a little, and at the same time to cover themselves with at least posthumous glory. Or, the expense might be borne by Government. A hundred and fifty thousand dollars down for building and outfit, with twenty-five or thirty thousand dollars annually for sustenance, would do very well. If it is right for Congress to equip transit expeditions in the interest of astronomy, it would certainly be right thus to assist the two sciences to which our greater industries are so deeply indebted. In fact, the United States can better afford to incur this very moderate expense than not to incur it. In the long-run the laboratory would be worth as much to the country as either the Naval Observatory, the Coast Survey, or the geological expeditions—all, by-the-way, excellent enterprises, which have received, if anything, less encouragement than they have deserved. The development of science in a nation means eventually the discovery of new resources and the creation of new wealth. Whoever doubts this statement needs only to look at the past achievements of physical science in order to be fully convinced of its truth. What national investment ever brought in richer returns than that famous grant made by Congress to S. F. B. Morse?

Should the proposed laboratory, if established, be independent, or connected with some other institution? That would depend upon a variety of circumstances. If endowed by the United States Government, it surely ought to be connected with and controlled by the Smithsonian Institution. Even an endowment contributed from private sources might well be placed under that management. For the Smithsonian Institution is really a magnificent example of a great trust splendidly administered. Both financially and as regards the interests of science the managers of this institution have done admirably. Money placed in their hands would certainly be well spent. Every dollar would be so handled as to produce the maximum good effect. A laboratory under this control, whether publicly or privately endowed, would assume a national character, and might serve as a centre of coöperation for investigators in all parts of our country.

Leaving Washington out of account, a laboratory for research might perhaps be best established in connection with some good university—like Harvard, Yale, Columbia, Michigan, or Cornell. It would then be already provided with a library and a building-lot, some apparatus at least would be ready to hand, and a strong social element would aid in the attraction of scientific men. Moreover, with such affiliations, the laboratory would often be able to secure good volunteer work from advanced or post-graduate students; an advantage by no means to be despised.

But it is hardly worth while to multiply suggestions. Enough has been said to show distinctly the main points in favor of a laboratory specially endowed for research, and some of the chief considerations which must arise in its establishment. Such a laboratory as is here indicated, a laboratory in which many specialists could combine forces in the more difficult fundamental investigations of physical science, surely ought not to remain long a mere fabric of the imagination, a misty dream of the future. It should be founded—whether by an individual or by the nation it matters little. Let us hope that, before many years pass by, the dream may become a reality.

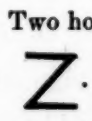
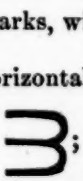
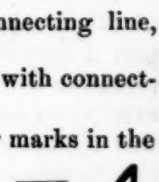
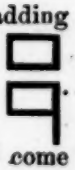
THE ORIGIN AND CURIOSITIES OF THE ARABIC NUMERALS.

By D. V. T. QUA.

IN an article on the "Origin of the Numerals," published in The Popular Science Monthly for January, 1876, the writer remarks: "Having never met with any explanation of the origin of the numerals, or rather of the figures symbolizing them, perhaps I am right in supposing that nothing satisfactory is known of it."

The history of the Arabic or decimal notation is somewhat as follows: The characters of this notation were introduced into Europe, during the tenth century, by the Crusades. From the Arabic, these characters have been traced to the sacred books of the Brahmans of India. It was long supposed that for our modern arithmetic we were indebted to the Arabians. But this, as we have seen, is not the case. The Hindoos communicated a knowledge of it to the Arabians, and we have been unable to trace it beyond the Hindoos: hence we must concede the honor to them of its invention.

To the Arabians, however, belongs the honor of introducing arithmetic into Europe. It was the Arabians who took the torch from the Orient and passed it along toward the Occident, when "westward the star of empire took its way."

The origin of the characters came, undoubtedly, from the fact that the Orientals first learned to count on their fingers and thumbs, and from this originated the *ten* characters employed, and originally called digits, from the Latin word *digitus*, signifying finger. In keeping accounts among the Orientals, one mark represented one finger, or number, thus: . Two horizontal marks, with a connecting line, stood for two, thus:  . Three horizontal marks, with connecting lines, would stand for three, thus:  ; and four marks in the form of a square, or a triangle, would stand for four, thus:  . Five marks in this form, , was the original figure five in this notation; six marks, thus, , the original figure six. The figure seven was made by marks representing two squares with one of the lines wanting, thus: . The figure eight was made by placing two squares near each other, thus: ; and nine by adding one more mark to the two squares representing eight, thus:  .

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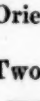
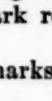
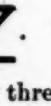
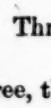
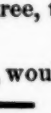
The origin of the characters came, undoubtedly, from the fact that the Orientals first learned to count on their fingers and thumbs, and from this originated the *ten* characters employed, and originally called digits, from the Latin word *digitus*, signifying finger. In keeping accounts among the Orientals, one mark represented one finger, or number, thus: . Two horizontal marks, with a connecting line, stood for two, thus:  . Three horizontal marks, with connecting lines, would stand for three, thus:  ; and four marks in the form of a square, or a triangle, would stand for four, thus: . Five marks in this form,  , was the original figure five in this notation; six marks, thus,  , the original figure six. The figure seven was made by marks representing two squares with one of the lines wanting, thus:  . The figure eight was made by placing two squares near each other, thus: ; and nine by adding one more mark to the two squares representing eight, thus: .

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39996 171392 779229

rangement seems to have come the *fundamental law* of the decimal notation in which its superior utility consists, and upon which quite recently has been based the metric system of weights and measures. By placing any of the digits in the place of the zero to make the numbers between ten and twenty, we have the law established. The science of arithmetic, like all other sciences, was very limited and imperfect at the beginning, and the successive steps by which it has reached its present extension and perfection have been taken at long intervals, and among different nations. It has been developed by the necessities of business, by the strong love for mathematical science, and by the call for its higher offices by other sciences, especially that of astronomy. In its progress, we find that the Arabians discovered the method of proof by casting out the 9's, and that the Italians early adopted the practice of separating numbers into periods of six figures, for the purpose of enumerating them. The property of the number 9 affords an ingenious method of proving each of the fundamental operations in arithmetic, and it seems to be an incidental attribute of this number. It arises from the *law of increase* in the decimal notation. It universally belongs to the number that is *one less* than the radix of the system of notation. And in this connection it may not be irrelevant to state some facts or curiosities with regard to this number 9. It cannot be multiplied away, or got rid of in any manner. Whatever we do, it is sure to turn up again, as was the body of Eugene Aram's victim. One remarkable property of this figure (said to have been discovered by W. Green, who died in 1794) is, that all through the multiplication-table the product of 9 comes to 9. Multiply any number by 9, as $9 \times 2 = 18$, add the digits together, $1 + 8 = 9$. So it goes on until we reach $9 \times 11 = 99$. Very well—add the digits $9 + 9 = 18$, and $1 + 8 = 9$. Going on to any extent it is impossible to get rid of the figure 9. Take any number of examples at random, and we have the same result. For instance, $339 \times 9 = 3{,}051$. Add the digits $3 + 0 + 5 + 1 = 9$. Take one more, $5{,}071 \times 9 = 45{,}639$, and the sum of the digits, $4 + 5 + 6 + 3 + 9 = 27$, and $2 + 7 = 9$.

The French mathematicians found out another queer thing about this number, namely: if we take any row of figures, and, reversing their order, make a subtraction, and add the digits, the final sum is sure to be 9. For example, $5{,}071 - 1{,}705 = 3{,}366$; add these digits $3 + 3 + 6 + 6 = 18$, and $1 + 8 = 9$. The same result is obtained if we raise the numbers so changed to their squares or cubes. Starting with 62, and reversing the digits, we have 26, then $62 - 26 = 36$, and $3 + 6 = 9$. The squares of 26 and 62 are respectively 676 and 3,844, and $3{,}844 - 676 = 3{,}168$; add $3 + 1 + 6 + 8 = 18$, and $1 + 8 = 9$. This may be exemplified in another way. Write down any number, as, for example, 7,549,132, subtract the sum of its digits $7 + 5 + 4 + 9 + 1 + 3 + 2 = 31$, and $7{,}549{,}132 - 31 =$

7,549,101. Add these digits, $7 + 5 + 4 + 9 + 1 + 0 + 1 = 27$, and $2 + 7 = 9$.

But we have extended already this article to a greater length than we intended, simply wishing to give the origin and history of the decimal notation as far as it can be traced, and will close by stating that this notation is every way adapted to the practical operations of business, as well as the most abstruse mathematical investigations. In whatever light it is viewed, the decimal notation must be regarded as one of the most striking monuments of human ingenuity, and its beneficial influence on the progress of science and the arts, on commerce and civilization, must win for its unknown author the everlasting admiration and gratitude of mankind.

THE SCIENTIFIC LABORS OF WILLIAM CROOKES.

AMONG the active and successful scientific workers of England, at the present time, the gentleman whose portrait we give this month is one of the foremost. Though only in the meridian of his manhood, he has made two discoveries—those of the metal thallium and of the radiometer—which will immortalize his name; while his minor labors in the field of science, both in the laboratory and in the editorial office, are in an unusual degree important and valuable.

WILLIAM CROOKES was born in London, in 1832. His scientific career commenced in 1848, when he entered the Royal College of Chemistry as a pupil of the distinguished chemist Dr. Hoffmann, now of the University of Berlin. He had gained the Ashburton scholarship at the age of seventeen. After two years of study, Dr. Hoffmann appointed him, first, his junior, and then his senior assistant, which post he held until 1854, when he went to Oxford to superintend the meteorological department of the Radcliffe Observatory. In 1855 he was appointed Teacher of Chemistry at the Science College, Chester. In 1859 he founded the *Chemical News*, and in 1864 he became editor of the *Quarterly Journal of Science*.

Mr. Crookes's researches were begun while at the Royal College of Chemistry, his first paper, "On the Seleno-Cyanides," being published in the *Quarterly Journal of the Chemical Society*, in 1851. Since then he has been almost uninterruptedly engaged in private research on subjects connected with chemistry and physics.

In 1861 Mr. Crookes discovered, by means of spectral observations and chemical reactions, the metal thallium; and in June, 1862, and February, 1873, he laid before the Royal Society an account of

its occurrence, distribution, and the method of extraction from the ore, together with its physical characteristics and chemical properties. He also discussed the position of thallium among elementary bodies, and gave a series of analytical notes on the new metal. In the *Journal of the Chemical Society* for April, 1864, he collated all the information then extant, both from his own researches and from those of others, introducing qualitative and quantitative descriptions of an extended series of the salts of the metal. In June, 1872, he laid before the Royal Society the details and results of experiments which had occupied much of his time during the previous eight years, and which consisted of laborious researches on the atomic weight of thallium.

In 1863 Mr. Crookes was elected a Fellow of the Royal Society. In 1865 he discovered the sodium amalgamation process for separating gold and silver from their ores. (This process was discovered independently, and at about the same time, by Prof. Henry Wurtz, of New York.) In 1866 he was appointed by the English Government to inquire into and report upon the application of disinfectants in arresting the spread of the cattle-plague then prevalent in England. In 1871 he was selected as a member of the English expedition to Oran for observing the total phase of the solar eclipse which occurred in December of that year.

Mr. Crookes commenced his research on "Repulsion resulting from Radiation" in 1872. These experiments were suggested by some observations made when weighing heavy pieces of glass apparatus in a vacuum balance during his researches on the atomic weight of thallium. His first paper on the subject was read before the Royal Society on December 11, 1873, and during the last three years Mr. Crookes has sent six other communications to the society on the same subject. The construction of the radiometer is one result of his investigation. At first it was thought that the movement of the vanes in the exhausted bulb was due to radiation, for no movement took place until the vacuum was so good as to be almost beyond the powers of an ordinary air-pump to produce, and as the vacuum got more and more absolute, so the force increased in power; but Mr. Crookes soon found that at a rarefaction so high that the residual gas was a non-conductor of an induction-current, there was enough matter present to produce motion, and therefore to offer resistance to motion. That this residual gas was not a mere accidental accompaniment of the phenomena was rendered probable both by the experiments of Dr. Schuster and by that of Mr. Crookes, on the movement of the floating glass case of a radiometer when the arms are fixed by a magnet, which was demonstrated to the Royal Society on March 30, 1876. Mr. Crookes has since constructed a special apparatus for measuring the vacuum. A vertical plate, instead of continuously ro-

tating in one direction, as in the ordinary radiometer, is suspended by a glass fibre, which it twists in opposite directions alternately. The movement is started by rotating the whole apparatus through a small angle, and the observation consists in noting the successive amplitudes of vibration when the instrument is left to itself, a mirror and spot of light being employed for this purpose. The results of these experiments leave no reasonable doubt that the repulsion is due to the internal movement of the molecules of the residual gas.

In 1875 Mr. Crookes received the award of a Royal medal from the Royal Society for his various chemical and physical researches; and in 1876 he was elected a Vice-President of the Chemical Society.

Previous to his researches on "Repulsion," Mr. Crookes began to investigate so-called spiritualism. As far as it extended, his inquiry into the subject convinced him that certain phenomena obtained under test conditions in his own house were due neither to tricks, mechanical arrangements, nor to legerdemain. He inclined to the opinion that the "medium" possessed what Mr. Sergeant Cox calls psychic force, but he had arrived at no definite conclusions as to the cause of the phenomena when he decided to discontinue their investigation.

Mr. Crookes is the author of "Select Methods in Chemical Analysis," of "The Manufacture of Beet-Root Sugar in England," and of a "Handbook of Dyeing and Calico Printing." He is also joint author of the English adaptation of Kerl's "Treatise on Metallurgy." He has edited and enlarged the last two editions of Mitchell's "Manual of Practical Assaying," and translated into English and edited Reimann's "Aniline and its Derivatives," Wagner's "Chemical Technology," and Auerbach's "Anthracen and its Derivatives."

It is claimed that Mr. Crookes was the first to apply photography to the investigation of the solar spectrum, but his earlier researches were so numerous that it is impossible to refer to them all. We may, however, mention his papers "On the Opacity of the Yellow-Soda Flame to Light of its own Color," "On the Measurement of the Luminous Intensity of Light," "On a New Binocular Spectrum Microscope," and "On the Optical Phenomena of Opals."

CORRESPONDENCE.

THE NATURE AND CAUSE OF FERMENTATION AND PUTREFACTION.

To the Editor of the Popular Science Monthly.

I HAVE read with no ordinary interest the lecture by Prof. Tyndall, published in your issue of December, upon the subject of "Fermentation and its Bearings upon the Phenomena of Disease;" and I desire, with your permission, to submit some points suggested to my mind upon which, according to my own conception, there remains some doubt, and which I should like to see explained. It is not my intention to dwell upon the general subject of the nature and causes of fermentation, but merely to touch upon it, confining myself rather to the question of the causes of putrefaction.

In fermentation and the production of alcohol, the presence of bacteria seems to be constant. Prof. Tyndall holds that the changes resulting in the formation of beer and the production of alcohol are due to the action of these microscopic germs, which, seizing upon the grain, or fruit, elaborate the spirit. Now, it does not seem to me perfectly clear that the changes which take place in the fruit may not be purely chemical, and that a portion of the component elements of the fruit, not requisite to form the chemical combination of alcohol, becomes a suitable soil in which the air-germs can take root and grow. It is a question of cause and effect.

Again, a certain amount of moisture is necessary to the production of mould. It may be asked: Is it quite certain that the moisture is not the agent of a chemical decomposition, and that the growth of mould is due to the deposit of seed in soil furnished by this decomposition? May it not be that a dry and cold atmosphere prevents or retards chemical decomposition in devitalized organic matter, while heat and moisture cause or facilitate it? Why should we disregard the chemical forces in the decomposition of organic matter deprived of vitality? Or, does Prof. Tyndall wish us to regard all organic matter as possessed of vitality until it is decomposed? Indeed, he says: "Cherries, apples, peaches, etc., are composed of cells, each of which is a living unit;" and "the living cells of fruit can absorb oxygen and breathe out carbonic acid, exactly like the living cells of the leaven of beer." We know that the seed of fruit possesses vitality; but is it proof of vitality in the cells, that certain changes take place between the constituents of the cells and the external air, and that other changes take place when the fruit is excluded from the air? Granting it is, then shall we say that all vegetable products, although long since uprooted but undecomposed, are possessed of life? Are the cells which compose the well-worn oak-beams of the few remaining wooden walls of Old England still endowed with vitality, and constantly engaged in a struggle for life with the low forms of animated Nature? Then, if this be conceded, might we not assume that fructification of germs is likewise essential to the decomposition of minerals? If the decay of an old boot is dependent upon the growth of mould, may we not suppose that the rusting of an old axe is due to similar influences; and that the erosion of rock, which in time forms abundant soil for vegetation, is the work of microscopic germs, although commonly supposed to be due to physical forces?

Prof. Tyndall says that "some of the numberless air-germs produce acidity, some putrefaction." But when acidity takes place rapidly, as in a frozen apple just thawed, and with an unbroken skin, are we still to regard it as the result of bacteria? With regard to putrefaction, Prof. Tyndall cites a number of experiments with beef-tea. Now, to make these perfectly satisfactory, it seems to me that the fluid ought to be exposed to not a limited quantity of pure air, but a free change of air from which all motes had been removed.

I now come to the subject of putrefaction in connection with the living body, and the antiseptic treatment of wounds as taught by Prof. Lister, of Edinburgh. Prof. Tyndall says that he has obtained "a specific against putrefaction and all its deadly consequences." This statement might lead the public to believe that the teachings of Prof. Lister were generally accepted by the medical profession. Such, however, is by no means the case, notwithstanding his practice has been thoroughly tried in most if not all the principal hospitals of Europe and America. While a certain number have adopted his method, the majority have rejected it, with the conviction that other treatment less troublesome is quite as, if not more, successful. That Prof. Lister's theories and practice are not believed in by the representative surgeons of the United States, was clearly demonstrated at the late International Medical Congress, at Philadelphia. Prof. Lister was

chairman of the surgical section, and he was courteously allowed all the time and opportunity he desired, which was a good deal, to fully explain his views. But, after all, the section refused to indorse his mode of treating wounds. The presence of germs in the air was not denied; the views of Pasteur were not disputed; but the question was, Have these theories, facts if you please, any necessary connection with putrefaction? The question was and is: Is it true, or not true, that putrefaction cannot take place without the presence of air-gems? Is it true that animal organic matter, when deprived of vitality, will remain undecomposed for an unlimited time unless bacteria seize upon it; or will it not, by a purely chemical process, decompose? Decomposing organic matter, we know by daily observation, is the abode of low, degraded animal life, and the soil in which low forms of vegetable life take root and grow; but are we to regard this as the cause or the result of putrefaction? If it be admitted that putrefaction may, under any circumstances, in the absolute absence of bacteria, take place as a chemical process, is it not begging the question to assert that their presence is ever necessary? Every surgeon knows that putrefaction does often take place in the body beneath an impervious skin. This fact was accounted for by Prof. John T. Hodgen, of the St. Louis Medical College, who was the surgical reporter at the recent congress, upon the subject of antiseptic surgery, on the supposition that the bacteria reached the place of putrefaction through the lungs, or stomach, and the blood. He also declared that bacteria had been found in wounds beneath Prof. Lister's most elaborate and carefully-prepared dressings; and that they must have found their way there through the blood. But Prof. Lister took occasion to repudiate this doctrine. He did not believe the germs arrived at the wound by way of the blood-vessels; and we can understand why he should reject this theory. The writer of this communication then pointed out the uselessness of Prof. Lister's antiseptic dressing externally, if bacteria could enter by another way. Prof. Lister not only denied this theory, but admitted that putrefaction did sometimes take place independently of bacteria as a chemical process. The writer then submitted, and it is submitted now, that if putrefaction ever takes place without the influence of bacteria, it is impossible to prove that it, in any case, depends upon their presence.

It is no uncommon experience of surgeons to see wounds heal rapidly without putrefaction, although no steps are taken to place a barrier to the entrance of air-germs, or to destroy those which may have lodged in the part. Undoubtedly the air is, sometimes, especially in badly-ventilated hospitals, loaded with germs of a specific and poisonous nature, which will contaminate any wound, as the poison of erysipelas, but that common unadulterated air is inhabited by organisms whose existence and operation are essential for putrefaction, remains unproved. The value of carbolic acid and similar agents is generally acknowledged by the medical profession. They are in constant use, but not with the view of destroying germs. They are found to possess the property of arresting or preventing putrefactive chemical decomposition — just as common salt has in preserving meat—and hence their usefulness in the treatment of wounds.

When Prof. Tyndall "passes the bounds of surgery and enters the domain of epidemic disease," and points out the analogy between *contagium* and fermentation, he gives utterance to views long held by the medical profession. That small-pox, scarlet fever, etc., are developed in the system by "indefinite self-multiplication of germs (zymosis) introduced from without," is a commonly-accepted doctrine.

In support of the statement that Prof. Lister's antiseptic method is not regarded as essential to the successful treatment of wounds, one fact may be given, although more might be furnished. Prof. Spencer, who occupies the chair of surgery in the Edinburgh University, and who is therefore a colleague of Prof. Lister's, and likewise the author of a highly-esteemed work on surgery, continues to treat wounds without reference to Lister's theories, with results quite as satisfactory as any claimed by Prof. Lister. WILLIAM CANNIFF, M. D.

TORONTO, *December* 12, 1876.

INSECTS AND FLOWERS IN COLORADO.

To the Editor of the Popular Science Monthly.

ALLOW me to express my thanks to Prof. Gray, and Mr. Putnam, in so kindly furnishing the facts I asked for in regard to the insects of Colorado. Since my inquiry was written, Prof. Gray has defined his position in the January number of the *American Agriculturist*. He does not contend for the general necessity of cross-fertilization as Lubbock, Wallace, and others have done, but simply that an occasional cross is beneficial. When flowers are not visited by insects, they generally self-fertilize. "'Cross-fertilize if you can, self-fertilize if you must,' is Nature's golden rule for flowers." Of course this narrows the question to the *benefits of an occasional cross*, and renders much that I have written no longer of account. Of this character is this question of the quantity of insects in Colorado.

If plants can "generally" fertilize themselves without insect aid, simply preferring cross-fertilization through insect agency when they can get it, the abundance or partial absence of insects there is of little consequence in the argument.

The question being purely entomological, and no longer of importance to the botanist, I should feel sorry for having put Mr. Putnam to the trouble of writing his letter, only that I know facts are always welcome to the lover of science, though they may have no immediate bearing on questions under discussion. THOMAS MEEHAN.

GERMANTOWN, PA., *February* 22, 1877.

THE MATHEMATICAL CONTROVERSY.

To the Editor of the Popular Science Monthly.

SIR: You will doubtless be gratified to find that the premise upon which you have rested a charge of disingenuousness against a gentleman no longer living is mistaken. The case is this: You find that the negatively-quantitative geometry of Spencer's first edition of his "Classification of the Sciences" must have been that branch of mathematics which has grown up under the name of "Descriptive Geometry;" and you find the late Mr. Chauncey Wright disingenuous in representing that Spencer had reference to those technical methods of geometrical construction to which engineers apply the name. But Wright, like you, understood Spencer to refer to Monge's descriptive geometry; and it was just that which he characterized as a mathematical art having no place among the abstract sciences.

Permit me to add that I used to talk with Wright, daily, while he was writing the article in which this matter is discussed; and I declare that nothing could less describe his method than to say that he "was hunting through Spencer's various books in search of flaws." On the contrary, no critic ever studied his author more conscientiously; and very few have succeeded as well as he did in comprehending thought remote from the channel of their own. The present case illustrates this, for Wright seemed to detect that Spencer had two very different things confounded together in his mind, viz., descriptive geometry and positional geometry. The second edition of Spencer's book makes it pretty clear that this is so; for some of his warm disciples maintain that he still means the former, while to the mathematician his present words describe with tolerable accuracy the latter.

No doubt, Wright greatly under-estimated the importance of Herbert Spencer's philosophy. This was natural, because he found in Spencer's fundamental doctrine of the universality of evolution a proposition radically opposed to his own theory that there is only an ebb and flow, in this respect, and no unending progress. But such sharp antagonism only serves to make his criticisms all the more instructive. Whatever there may be of extravagance in the claims which are made for Spencer will be overthrown in the course of the discussion which is sure to go on, and which he himself would be among the very last of men to deprecate. It would be strange, indeed, if it were to turn out that an encyclopedic system of philosophy had been produced, so perfect in its details as to satisfy specialists. But disputation clears the philosophic air, and can only serve to bring into the light and to sharpen the outline of all that is to abide in Spencer's system. In this point of view, I cannot agree with you that Mr. Spencer's distinguished candor has done him any harm, or has postponed the knowledge of the truth for which he is striving.

Mr. Wright occupied a position opposed to that of most modern mathematicians, in maintaining that positional geometry is not quantitative. This, however, is not a question of mathematics, but of logic: and it goes very deep into the theory of logic, too. But, while it does not concern the "mathematical expert," as such, one does not perceive that Mr. Spencer has proved himself so supremely the master of the philosophy of mathematics that we need be greatly anxious lest Mr. Cayley should have ventured to express himself on the subject, without proper study of what Spencer has said.

You well say that we here "encounter a difficulty which always arises when knowledge outgrows old definitions." Prof. Peirce, in his "Linear Associative Algebra," offers a definition of mathematics, the acceptance of which would not necessarily involve any decision of the question whether that geometry which is not metrical is quantitative or not. Although linear associative algebra is certainly not popular science, perhaps you will find his remarks of sufficient general interest for insertion. He says:

"Mathematics is the science which draws necessary conclusions.

"This definition of mathematics is wider than that which is ordinarily given, and by which its range is limited to quantitative research. The ordinary definition, like those of other sciences, is objective; whereas this is subjective. Recent investigations, of which quaternions is the most noteworthy instance, make it manifest that the old definition is too restricted. The sphere of mathematics is here extended, in accordance with the derivation of its name, to all demonstrative research; so as to include all knowledge strictly capable of dogmatic teaching. Mathematics is not the discoverer of laws, for it is not induction; neither is it the framer of theories, for it is not hy-

pothesis; but it is the judge over both, and it is the arbiter to which either must refer its claims; and neither law can rule, nor theory explain, without the sanction of mathematics. It deduces from a law all its consequences, and develops them into the suitable form for comparison with observation; and thereby measures the strength of the argument from observation in favor of a proposed law, or of a proposed form of application of a law.

"Mathematics, under this definition, belongs to every inquiry, moral as well as physical. Even the rules of logic, by which it is rigidly bound, could not be deduced without its aid. The laws of argument admit of simple statement; but they must be curiously transposed before they can be applied to the living speech and verified by observation. In its pure and simple form, the syllogism cannot be directly compared with all experience, or it would not have required an Aristotle to discover it. It must be transmuted into all the possible shapes in which reasoning loves to clothe itself. The transmutation is the mathematical process in the establishment of the law. Of some sciences it is so large a portion that they have been quite abandoned to the mathematician, perhaps not altogether to the advantage of philosophy. Such is the case with geometry and analytic mechanics. But in many other sciences, as in all those of mental philosophy and most of the branches of natural history, the deductions are so immediate, and of such simple construction, that it is of no practical value to separate the mathematical portion and subject it to isolated discussion.

"The branches of mathematics are as various as the sciences to which they belong, and each subject of physical inquiry has its appropriate mathematics. In every form of material manifestation there is a corresponding form of human thought, so that the human mind is as wide in its range of thought as the physical universe in which it thinks. The two are wonderfully matched. But where there is a great diversity of physical appearance, there is often a close resemblance in the processes of deduction. It is important, therefore, to separate the intellectual work from the external form. Symbols must be adopted which may serve for the embodiment of forms of argument, without being trammeled by the conditions of external representation or special interpretation. The words of common language are usually unfit for this purpose, so that other symbols must be adopted, and mathematics treated by such symbols is called *algebra*. Algebra is, then, formal mathematics." I am, etc., C. S. P.

We cheerfully give place to the foregoing, prompted as it is by the generous desire of the writer to speak for one who can no longer speak for himself. Yet it hardly appears how our correspondent has much improved his friend's case. He objects, on the strength of intimate acquaintance with Mr. Wright and the spirit of his work, to our remark that he "was hunting through Spencer's various books in search of flaws." We certainly are not entitled to speak of Mr. Wright's motives, any further than they can be fairly gathered from his writings. No fair-minded person, acquainted with Herbert Spencer's labors, can deny that Mr. Wright's criticism upon him, in the *North American Review* of 1865, was a very prejudiced piece of work. C. S. P. admits that he greatly under-estimated the importance of Spencer's philosophy, which he accounts for by the natural bias of one who entertains rival views in the same field. Yet the article was far less a judgment of the philosophy, of which but a single volume had then appeared, than an estimate, gathered from an examination of Spencer's various productions, of his competency to produce a philosophy. He undertook to measure the man, and, as we now understand, with a predisposition to underrate him. He, therefore, could not have approached his works in an impartial or judicial temper, but rather in a state of feeling which interested him in their defects. At any rate, if he was not in quest of flaws, it is difficult to see how it was that he found nothing else. Six years before Mr. Wright wrote, and before Mr. Spencer had published a word of his philosophy, several of the ablest men in England joined in an appeal to the Government to secure for him a position of trust, on the ground that he was eminently the man to do a great and special work for the advancement and organization of knowledge that should be a national honor; and now, a dozen years after Mr. Wright wrote, the sixth volume of his philosophical system is awaited with eagerness by the leading minds of the foremost countries in the world. That is to say, he is doing the work that English philosophers predicted long ago (on the basis of what he had already published) that he would do. His works must, therefore, have had some excellences, some elements of strength, which it was the duty of candid criticism to recognize. But Mr. Wright seems to have

seen in them little except evidences of "ignorance," "incompetency," "confusion," "inconsistency," "perverted terminology," "fanciful discriminations," and, massing together these faults, he winds up his article with the inference of Spencer's "incompetency for the further development of his encyclopedic abstractions." This looks very much to us like "picking flaws," under the inspiration of a not very creditable purpose.

As to the special case there can be no doubt that Mr. Wright made a charge of ignorance against Mr. Spencer, founded on misrepresentation. He says that in his mathematical classification Spencer "has given a prominence to descriptive geometry which might be regarded as arising from the partiality of the civil engineer for a branch of his own art, were it not that he says, 'I was ignorant of the existence of this, as a separate division of mathematics, until it was described to me by Mr. Hirst.'" The insinuation is that Spencer, though educated as a civil engineer, was unacquainted with the branch of mathematical art that is especially familiar to engineers. That this imputation was groundless may be proved by referring to the *Civil Engineer and Architects' Journal* for 1839–'40, where will be found conclusive evidence of Mr. Spencer's early and thorough familiarity with the subject. Among other original papers in the field of descriptive geometry there published will be found a beautiful original theorem which dates back to the time when Spencer was but seventeen. He was not, then, so ignorant as Mr. Wright intimated, and certainly not so grossly ignorant as to confound a practical art with an abstract science, as erroneously represented by his critic. Although the arts grow into the sciences, so that both often pass under the same name, it requires no great discrimination to separate them; and if a question could arise as to which is meant, the character of the discussion would sufficiently determine it. Mr. Wright was no more justified in assuming that, by "Descriptive Geometry," as he was dealing with it, Mr. Spencer meant "certain methods of geometric construction, useful in engineering," than that by "geometry" he meant the art of earth-measuring instead of the science, or by "chemistry" the art of manufacturing paints and dyes, instead of its scientific principles. By the term "descriptive geometry," employed, as it was, in its scientific significance, Mr. Spencer did not mean Monge's "Géométrie Descriptive" of a hundred years ago, in which theorems and their applications to drawing were mingled together, but he meant the branch of mathematical science which has since grown up under this title, while omitting all mention of the practice that gave it the name of Descriptive Geometry, and for which the title Geometry of Position is now substituted. As Mr. Wright suggests the alternative that Mr. Spencer may have meant something else than what he imputed to him as the basis of a charge of ignorance, it is fair to infer that he did not know what he meant; and if he had not been animated by a predisposition to make out a bad case, he would have abstained from taking up the point, or would have dealt with it in a different spirit. We desire to do no injustice to the memory of Mr. Wright, but, as his works are now brought forward in a collective and permanent form, they are the proper objects of criticism, and we have commented upon one part of them, solely in what we consider the interest of truth.

EDITOR'S TABLE.

INTERNATIONAL COPYRIGHT.

DR. C. E. APPLETON, of London, has an article in the February *Fortnightly* on "American Efforts after International Copyright," which gives a generally correct account of what has been done here within the last few years to promote that object, but which places us in a false position, which we do not care to occupy. The interest of the topic is such that a few remarks of correction and reminiscence are here proper.

There was a revival of interest in the subject in 1871, which began in an English discussion, when Mr. W. H.

Appleton, of New York, wrote to the London *Times* that he did not think the American people were opposed to a copyright which had no other object than to secure the recognition of the rights of property of foreign authors in American editions of their works. He said that such a copyright had never been asked for by England, but only an arrangement to protect English printed books, by which the foreign manufacturer could get protection on his stock of paper, printing, and binding, in the American market under cover of his authors' copyright. The publisher's share in the production of a book—in materials and labor, which have a cash value in the market—may be assumed as at least ten times greater than the share contributed by the author, and which is represented by his customary ten per cent. royalty. Such international copyright laws as have been demanded are, therefore, ten times more for the protection of foreign book-manufacturers than for foreign authors. "Disentangle your author from the publisher," said Mr. Appleton, "and let him present his own claims, and my opinion is the American people will not deny them." The fairness of this position was acknowledged by the English press, but the English publishers were silent. The English authors, on the other hand, or a large proportion of the most influential of them, conceded the justice of the case, and drew up a memorial asking for negotiations on the new basis.

This stirring up of the subject led to some efforts on this side, to carry out the fundamental idea which Mr. Appleton had presented, and which had been previously urged in this country. There was at first but very little objection to the plan, and there was a general expression of favorable feeling toward an adjustment on the basis proposed. But opposition was quickly and vigorously developed, because the parties interested in the frustration of the measure were few and powerful, deeply concerned, and able to concentrate a prompt and efficient opposition. This fact we think was not sufficiently calculated upon, and the practical issue was brought before Congress prematurely, in the shape of a bill embodying a copyright in behalf of foreign authors. More time should have been allowed, and systematic measures adopted to sift the question thoroughly before the American people. We had a society organized for the promotion of international copyright, but it was committed to the old plan, and threw the weight of its influence against the new measure.

In this unfavorable state of things, when the opportunity had been adroitly seized by the tacticians opposed to copyright to confuse and befog the public mind by proposing all sorts of projects, a congressional committee was appointed, who called a meeting on February 12, 1872, for the hearing of all parties interested. We attended it, and it was certainly a very funny affair. We had not been accustomed to the atmosphere of Washington, and were, therefore, but poorly prepared for the sarcastic intimations of parties who lived there, as to the greenness of the gentlemen who came to the national Capitol expecting to interest Congress on such a question as this in a presidential year. The subject was discussed by various speakers, and in his account of it Dr. Appleton says, "Prof. Youmans followed, urging the claims of British authors upon the singular ground that they were very badly paid in their own country, and desired American sympathy." This is a total mistake; we argued the question on no such grounds, and offered no hint of any such reason why international copyright should be secured.

We demanded it of Congress solely as a measure of justice, and there was need enough that this view of the case should be urged; for the discussion before the committee was in the last de-

gree wild and discordant. All sorts of projects and crotchets were thrust forward on all sorts of pretexts. A lawyer was there to represent the Harpers, who opposed international copyright, but for reasons which he admitted would carry with them the destruction of all copyright. He presented a letter from the publishers for whom he was arguing, in which they said the claims of authors were not to be considered, but only the interests of the people at large. But why the people, having taken the communistic hint, and plundered the authors for their own benefit, might not go through the Franklin Square establishment and help themselves, in the interest of cheap knowledge, was not stated. The idea that there was any principle of right in the matter, which it was the duty of Congress to recognize, seemed to be quite lost sight of. The game of defeating the measure for an author's copyright (which it was feared at first might succeed) consisted in bringing up a great number of rival projects to bewilder the question, and the game succeeded. The committee reported against any congressional action, on the ground that it was not called for by equity, while those who professed to be in favor of the measure could come to no agreement in regard to a plan.

It may be added that, while we made no statement before the committee as to English authors being poorly paid, there was a comparison of the American method of payment by a percentage on the sales, with the English system of giving the author half-profits. The American method was commended and the English declared to be one from which their authors suffered; for, while by the percentage plan the author always gets something if there are any sales, on the English plan he gets nothing unless there are profits. And as, first, on the great mass of books there are no profits, and as, second, the making up of the "cost" is entirely in the hands of the publisher, the authors are liable to be victimized by the policy. In proof of this, and in explanation of how badly the half-profit system works for authors, we read from a pamphlet by James Spedding, an English author, in which the whole thing is exposed. He wrote it as two magazine articles, and could not get them published, because the editors said they should thereby make enemies of the publishers. So Spedding published the papers himself. We, however, made no absurd attempt to get up sympathy for English authors because they may be badly used by some of their publishers at home.

THE ORDER OF NATURE.

Some of the questions propounded to us through the columns of the *Tribune* by the Rev. Dr. Deems we answered in the preceding number of the Monthly, and postponed, for lack of space, the consideration of the following:

"The professor says that 'Prof. Huxley's antagonists hold that the inflexible order of Nature may be asserted, perhaps, in astronomy, but they deny it in biology.' Will he be good enough to refer me to one of the professor's antagonists who 'holds' that opinion?"

We here made an affirmation concerning a class, and Dr. Deems challenges us to produce a single instance in which it is true, which may be taken as an emphatic way of expressing his disbelief in what we said. Recurrence to the matter satisfies us that, besides being true, the proposition is more broadly true than we affirmed it to be. That the order of Nature is a principle accepted only in part, is a view held, not only by those who are ranked as antagonists of Huxley, but by the great mass of people, including even the largest proportion of scientific men. We have received, from Mr. W. H. Walworth, of Monticello, Iowa, a letter of inquiry that so clearly brings out the

attitude of mind to which we referred, that an extract from it will be here useful. Mr. Walworth remarks:

"You say (in your critique of Dr. William M. Taylor): 'And here is the vital point between Prof. Huxley and his antagonists. It is a question of the validity of the conception of the order and uniformity of Nature. Prof. Huxley holds to it as a first principle, a truth demonstrated by all science, and just as fixed in biology as in astronomy. His antagonists hold that the inflexible order of Nature may be assorted, perhaps, in astronomy, but they deny it in biology. They here invoke supernatural intervention. Obviously there are but two hypotheses upon the subject: that of genetic derivation of existing species, through the operation of natural law, and that of creation by miraculous interference with the course of Nature. If we assume the orderly course of Nature, development is inevitable; it is evolution or nothing.'

"Very well; it is evolution or nothing. Now, if evolution is true in biology, as Prof. Huxley maintains, my inquiry relates to the matter of the beginning of that evolution, and the beginning of life. Scientists do speak of the beginning of life. Is there a new force or a new principle introduced at this point that scientists have in mind when they say 'the beginning of life?'

"If so, does that new force or principle come through a miraculous interference with the course of Nature? If not, is life in its beginning other or more than a fact, and its appearance a phenomenon both to be accounted for by evolution in conjunction with matter and its inherent forces and principles?

"Given: First, the planet without life (so called); afterward, the planet with life.

"Then, if evolution is the law of 'dead matter,' and at the same time the law of 'living matter,' is there any chasm between that evolution does not bridge over?

"In other words, if evolution is the law, is there any escape from the conclusion that the beginning of life (so called) is a product of it, unless we accept the hypothesis of miraculous intervention? And why is any scientist permitted to entertain that hypothesis (miraculous intervention), at this particular point in the course of Nature (the point of the beginning of life), while he claims the right to reject it at all the other points along the line?"

This last question implies, what is perfectly well known, that many scientific men, naturalists, and even advanced biologists like Mr. Darwin, do invoke miraculous agency to explain the origin of life upon earth; that is, after admitting generally the great principle of natural causation, at a certain point they throw it up as insufficient, and appeal to supernatural causation.

It is surely unnecessary to waste words here in showing that the conception of the order of Nature has had an historic growth; that in the early times all the operations of the world were explained on the hypothesis of supernatural agents which science has so far dispelled as imaginary that the great phenomena of the heavens, the changes of the crust of the earth, and even atmospheric disturbances, are now referred for explanation to the operation of inflexible and universal physical laws. Where explanation breaks down and difficulties remain, in these branches of inquiry, the course pursued is to attribute the unexplained effects to lack of knowledge, and to wait for further light from the sources that have already afforded it. Nor can it be necessary to multiply words in showing that it is not so in biology, the science which deals with the phenomena of life. When a formidable difficulty occurs here, such as explaining the origin of species or the first advent of living things upon the earth, there is no waiting, but the knot is cut at once by appealing to miraculous intervention—to causes that are above Nature and out of Nature, and which cannot be investigated. There are, indeed, but few, even in the circles of science, who avowedly maintain the inviolable supremacy of natural causes, here as elsewhere, in Nature. They assume it generally, but affirm its inadequacy to explain all efforts. How many are there who recognize man, in his origin, to be as strictly and essentially a part and result of the order of Nature as any other creature? Like

Wallace and Dana, they go nine-tenths of the way, and then fly the track. Nature may do a large amount of the lower work; but for the origination of the higher part of man we must appeal to agencies higher than Nature.

Our correspondent does not see the reason of this. He asks why scientists are to be permitted to invoke miraculous agency at the point of the introduction of life, while they reject it at all others along the line of its development. They can only do this at the expense of logical consistency, and, so far as they do it, are unscientific. If a scientist does not know how life began, he should say so; and if he cannot find out himself, he has only to leave the inquiry to others. He is bound to explain it rationally, as he explains other effects in Nature, or to suspend his judgment. It is futile for him to resort to any short-and-easy methods of solving the problem, for it still remains to be worked by the scientific method. The whole history of our knowledge of Nature reveals an immutable order, an invariable and indissoluble chain of causation; and this principle a scientific man is never at liberty to discard because a serious difficulty is encountered; and, as a scientific man, he is never at liberty to discard it at all. Men talk lightly of breaks and supernatural intrusions in the course of Nature, which was well enough ages ago, but is now forbidden by the very conception of what Nature is. For thousands of years nothing was known of natural laws; now they form the basal idea of its constitution. The innermost texture, the essence and spirit, and the very definition of Nature, are unbroken, continuous order. It is by this alone that we know it. It is not enough to say that law is universal. It pervades all Nature, and constitutes the very idea of it. Our intelligence is bound up with it, is a part of it, and we neither know nor can know of any exception or limit to the principle. Men undertake to say where the natural order stops, and the supernatural is reached, but they juggle with words; for, the moment the so-called supernatural is brought within the cognizance of reason, it ceases to be supernatural. The alternative and antithesis of natural order is not the supernatural, but disorder. As the Rev. Baden Powell well remarks, "If Nature could really terminate anywhere, there we should find not the supernatural, but a chaos, a blank —total darkness—anarchy—atheism."

As to the chasm of which Mr. Walworth speaks between dead and living matter, it is, of course, nothing more than a chasm in our knowledge, and none the less a chasm when bridged over by the hypothesis of miraculous interference. The lowest form of life, the material basis from which all living things are spun, is protoplasm; but if Nature can produce a Newton or a Shakespeare in a few years from a formless protoplasmic germ, and by the course of natural causation, why should we say that it is past her power to produce the raw material itself, and fly to the supernatural to account for its earliest appearance? Science cannot take the theological explanation here, any more than elsewhere in Nature, for, if these explanations had been accepted as satisfactory, there never would have been any science. The scientific problem of the origin of life is a recent one. It has not been solved, but what has been already done, so far from disheartening inquirers, only stimulates them to greater effort. Chemistry already begins to build up organic substances artificially in the laboratory, although such an idea was long scouted as hopelessly impossible. A few generations more of work may put a very different aspect upon this profound inquiry; but, even if it takes centuries, the question must be held as belonging solely to the province of reason, and to be solved in accordance with the natural laws of cause and effect.

TOM EDWARD'S BIOGRAPHY.

Our notice of the career of Thomas Edward, printed last month, has elicited much interesting and sympathetic comment from the press, accompanied in repeated instances with something like skepticism as to its verity, or the possibility that a man of such genius could have been so long neglected in a community claiming the slightest degree of civilized intelligence. The story will appear more incredible in this country, where we can hardly appreciate the intensity of the class-feeling that pervades British society. The open secret of the case is, that Edward was a laborer, and not a gentleman, and, belonging to the servile class, he was not recognized or aided by the people around him. Scientific men corresponded with him, but they were at a distance, and probably neither knew nor inquired anything about his personal circumstances. And so he was left to fight his course alone, which he did manfully and bravely, contented if he could only work. The world never heard of Banff before, and it will be now known more for its meanness toward a poor shoemaker than for any other cause. But what shall we say of the meanness of the reviewer who thinks that the world should not have been apprised of the career of this remarkable naturalist until after his death? The Banff people, it is to be presumed, will not offer much excuse for their neglect, but a reviewer can express regret that justice has been done him by a distinguished biographer, as if he grudged the man the satisfaction of being justly recognized in his declining years. It was not enough that he should never have been the recipient of any aid to facilitate his scientific studies, but he must be refused also that reward which is the spur of ambition to the highest natures, the sympathy and approbation of their fellow-men! And if there be a lower depth of meanness yet, a reviewer can find it. Although Edward had been battered through a career that would have killed most men, enduring privation and exposure until health gave way with the approach of old age, yet the critic of the London *Academy* fears that the effect of publishing this premature biography will be, that no more work can be got out of the old man. This is what he says:

"Interesting and valuable though this memoir of a self-helpful and a self-denying life undoubtedly is, we are not sure that Mr. Smiles would not have acted a wiser part in deferring its publication. For it is rather bold to assert of one who is but threescore and two, and has shown but few signs of mental decline, that '*hic jacet* is all that remains to be added.' Rather we would hope that the downward course of Edward's life, smoothed by the queen's recognition of his services, may be a long and useful one, and that, having survived the danger of unmerited neglect, he may be spared the harder trial of intrusive patronage, to which this premature biography is likely enough to expose him. It is a perilous precedent for a successful author to have set, and we could have wished, for the sake of others, that Mr. Smiles had denied himself the pleasure of forestalling the verdict of posterity, and had culled his last example of self-help from a career already concluded."

LITERARY NOTICES.

Notes on Life-Insurance. Third edition. Revised, enlarged, and rearranged. By Gustavus W. Smith. New York: D. Van Nostrand. Pp. 204. Price, $2.

This book is an attempt to unfold the "mystery and art" of life-insurance to the general reader; to put before him in simple form, rid, as far as may be, of technicalities, a statement of the data upon which life-insurance problems are based, and the methods by which they are solved. For fifty years and more the business has been prominently before the public. It has been urged and expounded with a zeal and persistency that have become proverbial, and the inference is natural that there ought, by this time, to be among the people at

large a respectable amount of acquired information about it. Such, however, is not the case. The savings of the frugal have been embarked to the extent of $2,000,000,000 in schemes which claim to be designed for their mutual protection, not upon informed and deliberate judgment, but in great part as the simple result of yielding to the blandishments of the canvasser, combined with a sublime faith in the efficacy of statistical tables and mathematical formulæ which were not understood. Recognizing the extent of this ignorance, and the discomfort and distrust to which it is likely to give rise, the author has tried to make the road to knowledge in this direction easier. He has put into reasonable limits, and into logical and accessible shape, the more essential information pertaining to the theory of life-insurance, which heretofore was only to be gleaned from rare and expensive books, which were quite out of the reach of the non-professional reader. That this was no easy task is a fact which should be taken into account when measuring the degree of his success.

The book is divided into two parts. Part I. is theoretical. The tables of mortality are given, and we are shown how from these are obtained the expectation of life, and—assuming a given rate of interest—the net premium and the reserve—or, as the author prefers to call it, the trust-fund deposit. The theory of annuities is discussed in a series of problems; and the mechanism of the commutation-tables—those working-tools of the actuary—is explained.

Part II. is devoted to the discussion of such practical considerations as: the general management of companies; stock and mutual rates; the various plans of insurance; gross and net valuations, which involve the relation of the companies to the State; surrender values; annual statements, etc. The views under these heads are sound and well put, and, if they were widely circulated and read, would contribute to the welfare of both managers and the insured, and help to put their relations upon a surer footing. Algebraical discussions and formulæ and tables are relegated to an appendix, where they can be mastered or omitted, as the reader may choose.

We warmly second the author's hope that this little volume will be widely distributed and carefully studied, but must confess we are not very sanguine about the latter. It is true, as he says, that it contains no science that is very abstruse; there is nothing which a man with a little arithmetic and less algebra may not master, but it does imply that he should put himself to the strain of a little application and study, and this is just what the majority of men are unwilling to undertake. To average an interest account is a very simple matter, but most men who have never learned or have forgotten how to do it will, when one is rendered to them, take the chances on its correctness rather than take the trouble to verify it, and insure themselves against loss.

The issue of this third edition is timely. The management of insurance companies is a matter over which the public mind is just now very properly exercised. We know of no other attempt to give the information required for its intelligent consideration, and, so far as Mr. Smith's book succeeds in throwing light upon the subject, it will be doing a good work.

THE FUNCTIONS OF THE BRAIN. By DAVID FERRIER, M. D., F. R. S. With numerous Illustrations. New York: G. P. Putnam's Sons. Pp. 323. Price, $3.50.

THIS work embodies the last considerable effort, made by experimental physiology, to unravel and explain the mode of action of that most complex and obscure of all mechanisms—the brain of animals and of man. The author wisely says: "We are still only on the threshold of the inquiry, and it may be questioned whether the time has even yet arrived for an attempt to explain the mechanism of the brain and its functions." Much, however, has undoubtedly been accomplished toward the attainment of this end, though the steps forward are slow, uncertain, and difficult. What can be positively gained by any special research seems so small in comparison with the complete problem to be solved as to be hardly worth the immense labor involved; yet there is a fascination in the inquiry, and a grandeur in the result aimed at which awakens the enthusiasm of inves-

tigators, and assures the continuance of indefatigable research. Dr. Ferrier's investigations led him to certain important conclusions regarding the localization of functions in the brain, which have been approved by some physiologists, and criticised by others, although all agree as to the value of his skillful and well-directed experiments. The chief object of this volume is to present the author's views of the bearing of his experiments, although it contains a concise and well-digested account of the functions of the cerebro-spinal system in general, with the view more especially of pointing out the mutual relations between the higher and the lower nerve-centres. Dr. Ferrier's work was elaborately reviewed and in some respects adversely criticised by Mr. George Henry Lewes in two numbers of *Nature*. We have no space to state the points in issue, but will give his estimate of the work as presented in the closing passage:

"My space is exhausted, and I have not been able to do more than criticise the main topic of Dr. Ferrier's book—and this not with the fullness which its importance demands. But if I have shown grounds for regarding the hypothesis of voluntary centres in the cortex as at any rate far from *proved*, and in doing so have had to adopt an antagonistic attitude throughout my review, I should not be just to him, nor to my own feelings of gratitude, if I did not, in concluding, express a high sense of the value of his work, full as it is of suggestions, and rich in facts, which no counter-facts can set aside. It will long remain a storehouse to which all students must go for material. It may be the starting-point of a new anatomy of the brain."

THE CARLYLE ANTHOLOGY. Selected and arranged with the Author's Sanction. By EDWARD BARRETT. New York: Henry Holt & Co. Pp. 386. Price, $2.

IT was an excellent idea to get together in one compact volume the best thoughts of Carlyle, for there is a better and worse in his writings, as well as in those of all other authors. He has produced a lot of books in his day, unequal among themselves, but all containing, here and there, brilliant and powerful passages, well deserving to be thus separated and brought together for entertainment and edification at odd hours. We suspect, indeed, that Carlyle will be longer remembered for these strokes of extraordinary insight than on account of his elaborate works, in the great bulk of which there is a prodigious amount of wordiness—a fault which he so hated in other people. His works are mountainous, brilliant with gilded peaks, but with great stretches of valley between. It was not a bad idea of Barrett's to truncate the upper cones, and get the peaks all together in a single book, and, if Carlyle approves of it, as he says he does, and must do, all readers will be pleased.

NEW ENCYCLOPÆDIA OF CHEMISTRY. Chemistry, Theoretical, Practical, and Analytical, as applied to Arts and Manufactures. On the Basis of Dr. Muspratt's Work. Parts XV. to XX. Philadelphia: Lippincott & Co. Price, 50 cents per number.

WE have already referred to this important work in very commendatory terms, and we may add that its character is well sustained to the later issues. It is not so much a dictionary of chemistry, in which the science is pulverized into a great number of fragments, and each placed under its alphabetical head, as a work in which the great leading subjects of chemical manufacture are taken up in succession, and treated in elaborate and exhaustive essays. The work is hence in no sense a rival of Watt's "Dictionary of Chemistry," which deals with the pure science rather than its practical applications to art and manufacture. The last installments treat of the subjects of dyeing and calico-printing, electro-metallurgy, enamels, ether, explosives, preservation of food, fuel, and gas. These topics are considered with fullness, and brought up to the latest results of scientific investigation.

ARCHOLOGY; OR, THE SCIENCE OF GOVERNMENT. By S. V. BLAKESLEE, Oakland, California. New York and San Francisco: A. Roman & Co. Pp. 164. Price $1.25.

THIS is a very good little essay on government, but there is hardly enough science in it to justify the author in inventing a new term to describe it. He points out the great strides of the modern physical sciences, and contrasts with them the little that has been done of this kind in the fields of abstract thought, "especially in the all-important science of government."

He says, "This failure of a scientific treatment has been most remarkable;" and, as an attempt to remedy this defect by the development of a distinct science of government, the following treatise has been prepared. The little book is systematic and suggestive, and its matter is well presented; but the writer, in our opinion, has very little true conception of what science is in its applications to this subject.

THE PLAINS OF THE GREAT WEST, AND THEIR INHABITANTS. Being a Description of the Plains, Game, Indians, etc., of the Great North American Desert. By RICHARD IRVING DODGE, Lieutenant-Colonel United States Army. With an Introduction by William Blackmore. Illustrated. New York: G. P. Putnam's Sons. Pp. 448. Price $4.

THIS is a lively, entertaining, and withal a very instructive volume on the Indians and Western life. Its author writes from observation and experience, and has a happy faculty of seizing the most striking and significant features in description, and representing them in vivid and forcible language. The work abounds in sketches of travel, delineations of camp-life, pictures of scenery, accounts of game, and episodes of sporting adventure. But its main and most important portion is that which is devoted to the religion, social life, habits, amusements, occupations, and what we may call the general natural history of the Indians. Appended to the volume is an instructive table of Indians living in the United States, omitting those in Alaska, with the numbers and locations of the tribes and fragments of tribes that still survive. The introduction by Mr. Blackmore gives some striking facts in regard to the destruction of the buffalo. He says that during the three years 1872–'74 four and a half million of these animals were destroyed, of which three million were killed merely for their hides. This is equal to the destruction of all the cattle in Holland and Belgium, and is as if in three years half the cattle of Texas, or all the cattle in Canada, had been carried off by a plague!

Mr. Blackmore quotes a passage from Bishop Whipple, on "Our Indian Policy," that furnishes an excellent example of the working of "American politics," and gives data by which we can compare the fruits of administration of the "best government on earth" with the miserable monarchy that rules on the other side of the St. Lawrence:

"One one side of the line is a nation that has spent $500,000,000 in Indian wars; a people who have not one hundred miles between the Atlantic and the Pacific which has not been the scene of an Indian massacre; a government which has not passed twenty years without an Indian war; not one Indian tribe to whom it has given Christian civilization; and which celebrates its centenary by another bloody Indian war. On the other side of the line are the same greedy, dominant, Anglo-Saxon race, and the same heathen. They have not spent one dollar in Indian wars, and have had no Indian massacres. Why? In Canada the Indian treaties call these men 'the Indian subjects of her majesty.' When civilization approaches them they are placed on ample reservations, receive aid in civilization, have personal rights in property, are amenable to law and protected by law, have schools, and Christian teachers send them the best teachers. We expend more than one hundred dollars to their one in caring for Indian wards."

THE APPLICATIONS OF PHYSICAL FORCES. By AMÉDÉE GUILLEMIN. Translated from the French by Mrs. Norman Lockyer, and edited, with Additions and Notes, by J. Norman Lockyer, F. R. S. With Colored Plates and Illustrations. London: Macmillan & Co. Pp. 741. Price $12.

FIVE years ago a sumptuous volume appeared in Paris, by Amédée Guillemin, which was translated into English by the Lockyers, and republished by Macmillan, under the title of "The Forces of Nature." It aimed to be a popular account of the great physical forces, gravity, heat, light, and electricity. By the aid of numerous and finely-executed engravings, it attempted to make clear the principles of pure science with no reference to their uses or applications to the practical arts. A companion volume has now appeared by the same author, editor, and publishers, which supplements the first, by taking up the ap-

plications of physical forces. The book is done in the same splendid style, and we should wonder where so expensive and luxurious a scientific book could find buyers, did we not remember that the two editions in French and English make it accessible to the largest portion of the readers of the civilized world. It is difficult to convey a just idea of the scope and detail of this work, with its thirty-eight elaborate chapters and its four hundred and sixty-seven admirable illustrations, and we can only say that it is the most elegant and exhaustive pictorial work on the applications of science in its great leading departments that has yet appeared.

THE BENCH AND BAR OF SARATOGA COUNTY; or, Reminiscences of the Judiciary and Scenes in the Court-room from the Organization of the County to the Present Time. By E. R. MANN. Ballston, New York: Waterbury & Innman. Pp. 391. Price, $2.

BESIDES its waters, which are of interest to chemists, geologists, and invalids, and its gay summer life, so dear to the devotees of fashion, which combine to make Saratoga famous, the place is also celebrated for its lawyers. A county which has given to the bench such men as Cowen, Walworth, Willard, Bockes, and Spear, and to the bar such pleaders as Hill, Reynolds, Porter, and Beach, certainly deserves to have its legal history written out, and Mr. Mann has accordingly done it in a very creditable manner. In giving a sketch of the legal profession of the county, which has of course been closely associated with the growth of its population, the author has brought out various points of incidental interest. It appears that about 1790, before Saratoga Springs or Ballston Spa had yet been heard of, the town of Ballston included all the western and northern portions of the county, stretching away toward the Adirondacks. In that year two men, named Palmer and Gorden, were candidates for the supervisorship of this extensive region. The election for that spring was called to be held in the Milton Hill meeting-house. The day was bright and balmy, and so it was suggested that the election take place outside the church, and one of the justices, taking a suitable position, declared the polls open. The votes were taken *viva voce*, and "the people" went strongly for Gorden, who had been supervisor for several years before. Palmer, seeing he had no chance, drew off one of the justices, quietly went into the church and opened another poll, where thirteen men voted for him. The town-meeting had been appointed to be held *in* the church, and so Palmer was proclaimed unanimously elected by the citizens of Ballston. Gorden protested, but Palmer was "counted in," the same as nowadays. A feud followed between these office-seekers, the public espousing the causes of the rivals, and promoting their ambition. The knavish trick of the town-meeting ended in making both men county judges, and in sending each for two terms to the National House of Representatives. It is evident that Saratoga early furnished an excellent soil for the production of lawyers.

The first court-house and jail were erected at Ballston Centre, at a cost of $6,500, and were ready for use in 1796. Justice was dispensed there for twenty years, when it was burned down, with the following accompanying circumstances: Raymond Taylor, the jailer, was a man very full of the dignity and importance of his position, and would tolerate nothing that derogated from it in the prisoners under his charge. One named Billings had ruffled the jailer's complacency by some disrespectful words, and so Taylor had him securely fastened to the floor by a large ox-chain, riveted round his body by a blacksmith, and riveted also to the sill of the floor. Another prisoner set fire to the wall of his cell to burn his way out, and, as the flames rapidly extended, efforts were made to rescue Billings, but they could not loosen the chain, and so he was consumed with the burning court-house. As a further illustration of how modern politics is developed from its early germs, it may be stated that there was a law forbidding the jailer to furnish the prisoners with lights, so Taylor arranged with another man to furnish them, and divided the profits.

In a history of nearly a hundred years, only two men have been publicly strangled in Saratoga County in the interest of justice, and the first of them, hanged in 1820,

is said by the author of this work to have been insane.

The chief contents of the volume, consisting of personal sketches, anecdotes, and accounts of lawsuits, are, as might be expected, only of local interest.

THE ART OF PROJECTING. A Manual of Experimentation in Physics, Chemistry, and Natural History, with the Porte-Lumière and Magic Lantern. By Prof. A. E. DOLBEAR, Tufts College. Illustrated. Boston: Lee & Shepard. Pp. 158. Price $1.50.

THIS is a valuable little volume for all teachers and professors who desire to cultivate the art of illustrating their numerous scientific subjects by the projection of optical images of objects upon screens for the inspection of classes, or lecture-room auditories. Full attention is first given to the construction of apparatus, much of which, the author says, can be extemporized; and the author then points out how a surprisingly large number of experiments can be performed with these instruments in numerous departments of science and art. The book is full of neat woodcuts which aid the text in the description of operations, and it seems a thoroughly well-executed manual for helping on the work of scientific instruction.

A TEXT-BOOK OF PHYSIOLOGY. By M. FOSTER, M. A., M. D., F. R. S., Prælector of Physiology, and Fellow of Trinity College, Cambridge. New York: Macmillan & Co. Pp. 559. Price, $6.

DR. MICHAEL FOSTER has here given to the world a first-rate book on physiology. He is a good investigator, and a clear, pointed, and vigorous writer, and, with excellent scientific judgment in presenting the proportions of a subject, he has prepared a volume trustworthy in exposition and agreeable in its style. It is designed for medical students, and does not aim to be elementary, as the author proposes to begin about where Prof. Huxley's physiology leaves off. In fact, he hopes his book may come to be considered as a kind of advanced companion to Huxley's smaller volume.

The work contains a few simple diagrams, but it cannot be said to be illustrated, and herein we are inclined to think the author has made a serious mistake. His reasons for it are as follows: "I have, moreover, given neither figures nor elaborate descriptions of physiological instruments and apparatus. These must be seen, not read about; the student can learn more by five minutes' inspection of a piece apparatus, especially one at work, than by hours of study of even the most expensive and finished pictures, and most detailed verbal descriptions." True, but shall we therefore give up illustrations? It is always better to see the thing itself, and it should be the first law of education to get at the thing itself, and not take a picture in place of it, *wherever that can be done.* But then figures do no harm; they may be still helpful to those who have had the opportunity of inspection, while in the case of multitudes who have no such chance the pictures are much better than nothing. No one can look over the fine and accurate illustrations of such a work as Flint's "Manual of Physiology," for example, without recognizing the great advantage that most students will gain by referring to them.

THE FLEETS OF THE WORLD. THE GALLEY PERIOD. By FOXALL A. PARKER, Commodore of U. S. Navy. New York: D. Van Nostrand. Pp. 235. Price, $5.

THIS neat and elegantly-illustrated volume is a kind of introduction to the history of naval warfare. It is devoted to an account of sea-fights in old times, and it cannot fail to be very interesting to all who have a concern in the subject. We have greatly admired the finely-executed illustrations of the lubberly old ships that were employed before the improved modern craft came into use.

PUBLICATIONS RECEIVED.

The Microscopist: A Manual of Microscopy. By J. H. Wythe. Third edition, rewritten and enlarged. Pp. 259. With Plates. Philadelphia: Lindsay & Blakiston. Price, $4.50.

Michigan State Board of Health, 1876. Pp. 254. Lansing: W. S. George & Co. print.

Acoustics, Light, and Heat. By William Lees, A. M. Pp. 299. New York: Putnams. Price, $1.50.

Smithsonian Report, 1875. Pp. 422. Washington: Government Printing-Office.

Properties of Continuous Bridges. By C. Bender, C. E. Pp. 150. Boiler Incrustation. By F. J. Rowan. Pp. 88. New York: Van Nostrand. Price, 50 cents.

"The Jukes:" A Study in Crime, Pauperism, etc. By R. J. Dugdale. Pp. 118. New York: Putnams. Price, 50 cents.

Origin of the Chinese Race. Pp. 30. Japanese Wrecks in the North Pacific. Pp. 23. Early Maritime Intercourse of Ancient Western Nations. Pp. 13. By Charles Wolcott Brooks. San Francisco: Reprinted from the Proceedings of the California Academy of Sciences.

The Stone Age in New Jersey. By Dr. C. C. Abbott. Pp. 134. With numerous Plates. Washington: Government Printing-Office.

The Science of Astronomy. Lecture by A. K. Bartlett. Pp. 36. Battle Creek, Mich.: The Author. Price, 50 cents.

Analysis of Milk. By E. von Baumhauer. Pp. 34. New York: J. F. Trow & Son print.

Wisconsin Geological Survey for 1876. By T. C. Chamberlin. Pp. 40. Madison: S. D. Carpenter print.

The *Chinese Scientific Magazine*. Monthly. Vol. I., No. 10. John Fryer, Editor, Shanghai (printed in Chinese). Price, 50 cents per annum.

Addresses before the St. Louis Academy of Science. By C. V. Riley. Pp. 16. St. Louis: R. R. Studley & Co. print.

The Index. Containing classified Index of Periodical Literature. Monthly. Pp. 16. New York: William Erving. Price, $1.00 a year.

Theory of the Radiometer. By William Crookes, F. R. S. Pp. 16. London, 1877.

Minnesota Normal School Board for 1876. Pp. 40. St. Paul: *Pioneer Press* print.

Polar Colonization. By H. W. Howgate. Pp. 40. With Chart. Washington: Beresford print.

Zoölogical Society of Cincinnati. First, Second, and Third Annual Reports. Cincinnati, O.: Printed for the Society.

Common-School Education. By B. A. Hinsdale, A. M. Pp. 38. Cleveland, O.: Robinson, Savage & Co. print.

Metric System of Weights and Measures. Pp. 12. Boston: The Society of Civil Engineers.

Vitality of Certain Land-Mollusks. By R. E. C. Stearns. Pp. 2. With Plates. From the Proceedings of the California Academy of Sciences.

Report of the Commissioners of the State Survey for 1877. Albany: Parmenter print.

Outlines of Field Geology. By Prof. Geikie. Pp. 61. Price, 25 cents. Absorption of Light. By Prof. Stokes. Pp. 43. Price, 20 cents. London and New York: Macmillan.

Excrescences and Eccentric Wood-Growths in the Trunks of Trees. By Thomas Meehan. Pp. 6. From the Proceedings of the Philadelphia Academy of Natural Sciences.

Milk-Adulteration in the New York Courts. Pp. 32. New York: J. F. Trow & Son print.

POPULAR MISCELLANY.

Tyndall and Roberts on Spontaneous Generation.—Dr. Bastian, in a communication to the Royal Society of London, last June, cited some experiments to show that, while an acid urine usually remains barren after being boiled a few minutes, it becomes fertile when similarly treated if previously neutralized by liquor potassæ, especially if it be afterward maintained at a temperature of 115° or 120° Fahr. But the significance of these results for the doctrine of spontaneous generation is proved to be very little indeed by Dr. William Roberts and Prof. Tyndall, both of whom show that Bastian's experiments only confirm the observation made by Pasteur more than fourteen years ago, that alkaline liquids are more difficult to sterilize than acid ones. They further show that such liquids, once effectually sterilized, according to methods which they describe, remain perfectly sterile when the access of life-germs from without is precluded. The addition of the alkali appears to enable the preëxisting germs in the urine to survive the process of ebullition. To prevent this conservative action of the liquor potassæ, and at the same time to have a mixture precisely the same as that experimented on by Bastian, Tyndall adopted the following mode of procedure, which is substantially identical with that adopted by Dr. Roberts: Small tubes, with their ends finely drawn out, were charged with a definite amount of caustic potash, and subjected for a quarter of an hour to a temperature of 220° Fahr. They were then introduced into flasks containing measured quantities of urine. The urine being boiled for five minutes, the flasks were hermetically sealed during ebullition. They were subsequently permitted to remain in a warm

place sufficiently long to prove that the urine had been perfectly sterilized by the boiling. The flasks were then rudely shaken, so as to break the capillary ends of the potash-tubes and permit the liquor potassæ to mingle with the slightly acid liquid. The urine thus neutralized was subsequently exposed to a constant temperature of 122° Fahr., which is pronounced by Dr. Bastian to be specially potent as regards the generation of organisms.

Ten flasks, prepared as above described toward the end of last September, remained perfectly sterile for more than two months. There is no doubt that they would have remained so indefinitely.

Three retorts, moreover, similar to those employed by Dr. Bastian, and provided with potash-tubes, had fresh urine boiled in them on the 29th of September, the retorts being sealed during ebullition. Several days subsequently, the potash-tubes were broken, and the urine neutralized. Subjected for more than two months to a temperature of 122° Fahr., they failed to show any signs of life.

The Phenomena of Hypnotism. — Dr. Heubel, in *Pflüger's Archiv*, rejects Czermak's explanation of hypnotism (*see* THE POPULAR SCIENCE MONTHLY, vol. iii., p. 618, vol. iv., p. 75), as also the explanation offered by Kircher and Preyer, and thinks that all previous investigators of this phenomenon have witnessed only its first stage —that which is most easily induced in animals of relatively high organization. Cold-blooded vertebrates, such as the frog, may be reduced to a state of complete immobility at will; they will remain in a constrained position for hours, instead of seconds or minutes. This abolition of voluntary movement and of consciousness is, according to the author, nothing but ordinary sleep. He holds that the waking state requires for its maintenance a continual stimulation of the higher nervous centres by impressions conveyed to them along the various centripetal nerve-fibres. By forcing an animal to remain motionless for a brief interval (without inflicting pain), and simultaneously excluding visual and auditory sensations from its brain, we suddenly deprive its nerve-centres of a large proportion of their accustomed stimuli. Accordingly, they are unable to remain awake, and their functional activity is only restored to them when they are roused by some impulse from without. Having satisfied himself in a variety of ways of the correctness of this explanation as applied to the phenomena exhibited by the frog, Heubel proceeds to extend his results to birds and mammals, and arrives at the conclusion that "forced sleep" will account for all the facts hitherto observed.

Further Experiments with Putrescible Fluids.—Mr. Dallinger has communicated to the Royal Microscopical Society of London some further results of his experiments with sterile putrescible fluids. In these experiments, an air-chamber after Tyndall's plan was used, and it was tested for motes by a beam of oxyhydrogen-light. The germs were obtained from a maceration of haddock's head that had been kept for fifteen months, and found to contain numbers of the "springing and calycine monads" of former papers, many of them in a condition for emitting spores. A portion of this material was evaporated at the temperature of 150°. Dust from it was diffused through the Tyndall chamber, and, after the heavier particles had settled, in the course of four and a half hours, ten small glass basins filled with Cohn's nutritive fluid, freshly prepared, were introduced, six being open and four covered with glass lids. In this condition they were left for twenty-four hours, and then the lids were removed from the four covered vessels. After four days, "calycine" monads were found in all the first six vessels, and, in smaller numbers, the "springing" sort. Two days later the four vessels were examined; in three there were no calycine monads, and very few in the fourth; all exhibited the springing monads. The reason of this is probably to be found in the fact that the germs of the calycine monads are larger than those of the springing sort, and settled down first from their state of suspension in the air.

A Solar Distillery.—M. Mouchot lately described, at a meeting of the Paris Academy of Sciences, a very convenient solar alembic. The mirror is fifty centimetres in diameter, and the kettle holds one litre of

wine, which begins to boil on being exposed to the sun for not over half an hour. The vapor of alcohol is then condensed in a worm. The brandy thus obtained is very agreeable in flavor, no matter what kind of wine is used. It possesses an aroma resembling that of Kirschwasser. "It suffices," adds M. Mouchot, "to fill the kettle with water, and then to interpose between it and the worm a receptacle containing sweet-smelling leaves and flowers, in order to obtain all the essences yielded by distillation."

The Florida Cockroach new to American Science.—It is somewhat remarkable that, in certain parts of Florida, living is made almost impossible from the presence, in amazing numbers, of a cockroach not known North. The queer thing is that, while this pest has been long known in Florida, the fact has escaped the knowledge of scientific men. Mrs. Treat lately sent specimens to Prof. S. H. Scudder, the orthopterist, who was surprised to see them, and pronounces them the *Periplaneta Australasia* of Fabricius.

Meteorological.—A sixth paper by Prof. Loomis is published in the *American Journal of Science* for January, giving "results derived from an examination of the observations of the United States Signal Service." The object of this important series of papers is to generalize results, using as data the vast amount of observations made in all parts of the United States.

In this paper, Prof. Loomis considers first the period of unusual heat which occurred in June, 1873. The thermometer rose to 108° at one point—Fort Sully—and to 95° and 100° at other points, for several days in succession, indicating a temperature 20° above the mean for the month. The heated area was north of latitude 39°, and east of the Rocky Mountains, and advanced slowly eastward to Western New York.

It appears that the heat was over a well-defined area, which was also an area of depressed barometer. There was also a gentle movement of air from the south into that area, which accounts for some of the excess of heat; but the region where it arose—Colorado, Montana, and contiguous districts—was excessively dry. No northern winds occurred to cool the air, and Prof. Loomis thinks the great excess of heat may be attributed to the hot south winds already referred to, and, secondly, to the accumulated effects of the sun's radiation.

In the second part of the paper the movements, form, and distribution of rain areas south of latitude 36° are considered. When two or more inches of rain falls within eight continuous hours, we have a "great rainfall." It appears that such rainfalls do not usually continue more than eight hours, and only very rarely do they continue twenty-four hours, either at one station or at successive stations.

It is shown that, on the Gulf and Atlantic border, the great rainfalls are twice as frequent on the coast as at 200 miles inland from it. A cause assigned is the rising of the air from the ocean as it impinges upon the land, and the consequent condensation of its vapor. This movement of the air assumes a cycloidal direction, as was found to be the case in a great number of instances, the motion being from right to left, in the direction contrary to that of the hands of a watch. "Hence, every great rain-storm should be accompanied by an inward and cycloidal motion of the air."

In the distribution of fifty-two cases of great rainfall by seasons, it was found that forty occurred in summer and autumn to twelve in winter and spring. Northward of latitude 36° the difference was still greater, being as five to one. It is thus shown that great rainfalls are most frequent when the sun's heat is greatest, and the air contains most vapor.

The hours of the day have a direct relation to great rainfalls. Thus, they most frequently occur before 4.35 P. M., and seldomest at 11 P. M.; only eight out of fifty-two instances are reported by the night observations made at 11 P. M.

The area of greatest rainfall is found to be within that of the cycloidal movement of air, but not at the centre of low pressure. It is almost invariably eastward from it, sometimes more than 250 miles. Thus, a storm-area, as previously shown by Prof. Loomis, usually assumes an oblong shape, the long radius of which is ahead of the

storm, in or near the general direction of its motion.

At stations northward of latitude 36°, observations show that great rains are accompanied by easterly winds; but, at the more southern stations of the district, the winds are north of east; while, at the northern stations, the winds are from south of east. When the wind blows from any other quarter, it is usually light.

This paper, like others previously published, presents, with the diagrams which have been published with them, the general phenomena of atmospheric movements with clearness and precision, and will speedily supersede the vague speculations concerning them which have so much occupied the public mind.

A Rapacious Fish. — The *Serrasalmo piraya*, found in all the rivers of Guiana, is doubtless one of the most voracious of fishes. The genus *Serrasalmo* (literally "serrated salmon," because of the double row of serratures on the belly) can hardly be classed with *Salmonidæ*, from which they differ both in general appearance and in habits. The *S. piraya* is a small fish, seldom exceeding one foot in length, but yet there is no animal that it will not attack, man not excepted. Alligators, horses, as well as fishes oftentimes ten times their own weight, are preyed upon by the pirayas. In attacking a fish they begin at the caudal fin, and the victim, being thus left without the principal organ of motion, is devoured with ease, several pirayas sharing in the meal. They often bite a piece out of a horse's leg when passing through the water. The feet of ducks and geese which are kept in the neighborhood where pirayas are plentiful, are almost invariably cut off, and the young ones devoured. In such localities it is unsafe to bathe, or even to wash clothes, in the river, many cases having occurred of fingers and toes being cut off by them. Schomburgck, in his "Travels in South America," from which most of these particulars have been derived, states that these fishes are "caught with hook and line, and their greediness is so great that no art is necessary to conceal the bait. The hook may be baited with a piece of fish, bird, or animal, or merely their entrails; the piraya will dart at it the instant it is thrown into the water, and seize it with eagerness, but it frequently happens that with its sharp teeth it bites the line, and escapes with the hook in its mouth. We, therefore, surrounded the line where it was fixed to the hook, the length of two or three inches, with tin or lead, and though it had a clumsy appearance we were not less successful. Some precaution is necessary even after the fish has been lifted out of the water, or it will inflict in its struggles serious wounds; the angler has, therefore, a small bludgeon ready, wherewith its skull is broken."

Science and Ventilation.—Sundry members of the Paris Academy of Sciences, at a recent session, expressed themselves very strongly as to the defective ventilation of the hall in which their meetings are held. Said M. Bouley: "The air here is unfit to breathe; the thing admits of no excuse; instead of gas, I wish we had again candles, as in former times." M. Leverrier: "I asked for lighting with gas, but I had also asked for another mode of ventilation; but, with regard to this, there has been no change. However, General Morin is a member of the Academy, and, in eight days, proper apparatus for ventilation might be set up, if we so wished." General Morin: "Eight days! Ten years ago, the setting up of such apparatus was in principle decided on." Leverrier: "The present condition of things is simply disgraceful; no other hall is so badly ventilated as the hall of the Institute." The eminent astronomer, were he to inspect critically the assembly halls of scientific and legislative bodies in other countries, would doubtless find abundant reason for retracting this severe judgment.

Intestinal Calculi in Horses.—In England and Continental Europe large numbers of horses die annually from the effects of calculi in the large intestine or in the cæcum. Of these calculi, Dr. T. L. Phipson writes in the *Chemical News* that they often begin by being triangular, or sometimes perfectly square, with rounded edges and corners, and become finally circular. In all cases they are formed of highly-crys-

talline concentric layers, and attain to eighteen or twenty inches in diameter. This, he thinks, is the greatest size they can attain. When so large as this, they press out the sides of the intestine, producing inflammation and violent pain, which causes the animal to roll about in agony, and, sooner or later, kills him. They consist mostly of phosphate of ammonia and magnesia, and the amount of organic matter is not great. This salt the author refers to the grain fed to the animals, and he raises the question whether grain is not for the horse a highly-artificial food. He is of the opinion that repeated doses of very dilute hydrochloric acid, say two to five per cent., in water or spirit, if it can be made to reach them, would quickly destroy the largest of these calculi. The lime in the water drunk by horses has nothing to do with the production of these concretions. It originates in the food, and is, in a large measure, due to a want of salt in the grain. Hence, working-horses that are highly fed should have lumps of salt to lick, and have salt in their food, and plenty of water to drink. The ventilation and drainage of stables is another important consideration. Many valuable beasts, after a hard day's work, pass the night in an atmosphere loaded with fumes of ammonia.

Abnormal Fruits.—Some abnormal fruits of the pear-tree, in appearance like very large acorns, having been exhibited at a meeting of the Academy of Natural Sciences of Philadelphia, Mr. Meehan took occasion to explain that a fruit is a modification of both leaves and branch. When a bud, he said, is being formed in the apple, pear, or similar trees, it may finally be either a flower-bud or a bud producing a new branch. Varying phases of nutrition decide this question. Exactly the nature of this variation we do not know; but we do know that the growth-force in the bud is arrested by some law of nutrition, and, instead of an elongated branch, what would be its series of spirals are drawn together closely, and the whole modified and made to form a flower. Thus, in the pear, it takes five buds to form one full cycle on a branch. When growth is arrested to form a flower, this first cycle is transformed into a five-lobed calyx, and generally this becomes much enlarged and fleshy, and covers all the other cycles of buds, which go to make up the inner layer of flesh terminating in the petals, carpels, or core, and so on. In the case under consideration the arresting force was imperfect. It had succeeded in forming the outer or calycine verticillate series of buds into a fleshy matter, giving what here might be called the cup of the "acorn;" but then the accelerating or branch-producing force gained a temporary advantage, and pushed on, forming the acorn-like centre, but only to be soon again arrested. This abnormal pear was, indeed, nothing more than an effort of the tree to produce a branch after a fruit had been decided on—a struggle which was finally decided in favor of the fruit.

Explanation of the Ball-Paradox.—Reuleaux offers the following explanation of the curious phenomenon of a ball being supported in air by a strong air-current directed obliquely upon it at an angle of 35° to 40° from the vertical: The pretty thin air-current, on reaching the ball, is deflected on all sides, and therefore more or less rarefied in its interior. Accordingly, the atmosphere presses the ball in the direction of greatest rarefaction, or the mean force of the rarefactions, toward the orifice. The weight of the ball acts vertically downward. Equilibrium occurs between the obliquely acting force of the current and the two forces just named, when the mean force of the latter is parallel to the action of the current. This can only take place when the ball has its centre under the axis of the current. There are then two forces which put the ball in rotation. If the finger or a rod be brought to the place of supposed minimum pressure on the ball, the latter is forthwith driven off (the vacuum being destroyed), or falls down.

Successful Case of Transfusion of Blood.—A case of successful transfusion of blood is recorded in the *Lancet*. The patient, a clerk, twenty years of age, was completely demented, hyperæmic, anæsthetic, and cataleptic; refused all food; dribbled constantly. The pulse was very feeble, rate 70, respiration 24. His state was one of profound

anæmia. A student in St. Thomas's Hospital volunteered to supply the blood for the operation. The patient received 200 grammes of blood without showing any bad symptoms; he even gave evidence of being roused from his habitual torpor. Three hours after the operation, the patient, who had, in the mean time, been placed in a warm bed, and had taken doses of tea and brandy, had a full pulse, rate 90, respiration 28. He answered to his name and spoke a few words, rubbed his face with one of his hands, opened his eyes, and swallowed voluntarily. Five hours later the pulse was 100, strong, respiration 30. The following day the pulse was 96 and respiration 28, and the patient ate and drank well and often. Toward evening the pulse was 90, respiration 28, and he spoke and answered slowly when spoken to; said he had no pain. Four days later the symptoms still continued to be favorable. The process of transfusion was to be repeated by the physicians, the results being so encouraging.

Production of Sulphurous Acid for Use as a Disinfectant.—Sulphur-fumes (sulphurous acid) have from time immemorial been employed to fumigate and purify infected air, but the ordinary method of producing the fumes by burning sulphur is cumbrous and very uncertain. Mr. T. W. Keates offers in the *Lancet* a ready and simple means of effecting this object. Instead of sulphur, he proposes to use bisulphide of carbon, a compound consisting of two atoms of sulphur and one of carbon. It is a dense, mobile liquid, heavier than water, and intensely inflammable. During combustion the constituents of the bisulphide combine with the oxygen of the air, producing sulphurous and carbonic-acid gases, the former greatly exceeding the latter in quantity. The bisulphide can be burned in a common spirit-lamp, or it may be mixed with oils and burned in an oil or kerosene lamp. Any proportionate quantity of sulphurous acid can in this way be thrown into an atmosphere, and the action may be continued for any length of time. As bisulphide of carbon is extremely volatile, the lamp should be furnished with a well-fitting screw-cap, to prevent loss by evaporation.

A Fishing-Spider.—"Just before the late war," writes the author of a communication in the *American Naturalist*, "I was at Colonel Oakley Bynum's spring, in Lawrence County, Alabama, near the town of Courtland, where I saw a school of minnows playing in the sunshine near the edge of the water. All at once, a spider, as large as the end of my finger, dropped down among them from a tree hanging over the spring. The spider seized one of the minnows near the head. The fish thus seized was about three inches long. As soon as it was seized by its captor, it swam round swiftly in the water, and frequently dived to the bottom, yet the spider held on to it; finally, it came to the top, turned upon its back, and died. It seemed to have been bitten or wounded on the back of the neck near where the head joins. When the fish was dead, the spider moved off with it to the shore. The limb of the tree from which the spider must have fallen was between ten and fifteen feet above the water. Its success shows that it had the judgment of a practical engineer."

Qualitative Determination of Potassa.—Carnot offers a new and simple process for the qualitative detection and the determination of potassa, hitherto one of the most delicate operations in analytical chemistry. It is as follows: In a few drops of hydrochloric acid, one part of the subnitrate of bismuth, say half a gramme, is dissolved, and then, in a few cubic centimetres of water, are dissolved about two parts (one gramme to one and a quarter) of crystallized hyposulphite of soda. The second solution is poured into the first, and concentrated alcohol added in large excess. This mixture is the reagent. If brought in contact with a few drops of the solution of a potash salt, it at once gives a yellow precipitate. With an undissolved potassic salt it produces a decidedly yellow coloration, easily recognized. All potassic salts with mineral acids are susceptible of this reaction; it is also very sensitive with the organic salts—tartrates, citrates, etc. The reaction is not interfered with by the presence of other bases, with which nothing analogous is produced. The character is therefore perfectly distinct. Baryta and

strontia alone may occasion some difficulty by reason of the white precipitates of double hyposulphites, which they form with the same reagent; but it is very rare to meet them along with potassa, and they are very easily detected and removed.

Prevention of Fires in Coal-Mines.—In an address on fires in mines, Mr. Richard P. Rothwell affirms that the most efficient preventives of such fires, from whatever cause they may come, are to be found in education, in increased knowledge of the causes of fires, and a better appreciation of the working of these causes. Mine-managers he would compel to undergo strict examinations, nor would he allow any one to undertake the responsible duties of this place without a certificate of competency from a qualified board of examiners. He would not, however, stop here, but would have the miners themselves instructed as to the causes and preventives of the dangers they meet with in their work. Special free instruction upon these points might be furnished at every colliery; and this could doubtless be accomplished by encouraging the giving of popular lectures, by practical miners and engineers, on subjects of interest to the miner, and by giving small prizes to those who pass the best examination on subjects of daily practical application in their calling. Greater knowledge always makes better workers, and mine-owners would find in this a good return for the expense incurred.

Economy of the Electric Light.—In a series of experiments on electric light, Prof. W. A. Anthony used an electro-magnetic machine of the Gramme pattern, driven by a five-horse Brayton petroleum-engine. The engine consumed a little over 6¼ pounds of crude petroleum per hour. The lamp used in the engine, by which the explosive mixture is fired, had a one-inch flat wick, and consumed 29.8 grammes (459 grains) of oil per hour. The power resulting from the motion of the engine, when applied to the electric machine, produced a stream of electricity or electric light having an illuminating power equal to that of 234 of the lamps mentioned, showing that three times more light may be produced from a given quantity of oil, if its energy is converted first into mechanical power and then into electricity, than if the oil is directly burned in a lamp.

Southern Illinois Academy of Science.—The Southern Illinois Academy of Science, a newly-founded scientific association, with its seat at Carbondale, has for its objects the investigation—1. Of the ethnology and history of Southern Illinois, including its antiquities and aboriginal remains; 2. The geology, botany, and zoölogy, of that section; and, 3. To encourage the production of original papers on the above, and on special mathematical, astronomical, and meteorological subjects, as well as on the origin and meaning of the names given to localities by the Indians and the first white settlers of the country. The Academy is engaged in making a collection of materials illustrative of the field of research to which it has devoted itself, and has issued a circular calling for contributions of archæological and aboriginal remains, historical notes, maps, sketches of mounds, natural history specimens, etc. The Secretary of the Academy is Prof. Granville F. Foster, Carbondale, Illinois.

Threatened Eruption of Mount Vesuvius.—For many weeks Mount Vesuvius has been threatening an eruption. Prof. Boyd Dawkins, who visited the volcano in January, found, on arriving at the mouth of the crater, that it was filled with dense vapor like a fog. A low, roaring noise could be heard, and occasionally there was a flash of light, probably the reflected glare of the lava surging about in the volcano. Undismayed by these symptoms of internal disturbance, Prof. Dawkins went down seven or eight feet below the crater's edge, and found that he could light pieces of paper in holes which he dug with his hammer in the black ash on the inside. He is of the opinion that Vesuvius performs the duty of a safety-valve to a very large portion of the earth. At present the mountain is in a very restless state, and there may be an outbreak at any moment. The event is looked for with great interest by the inhabitants of Naples, as it will bring sight-seers from all parts of the world to their city.

The Challenger Collection.—The collections of marine animals made by the Challenger Expedition are declared by Prof.

Agassiz to be in a better state of preservation, and their localities more accurately noted, than is the case with any similar collection he has seen. To give an idea of the magnitude of the Challenger collections, he says that if a single individual, possessing the knowledge of the eighteen or twenty specialists in whose hands they are to be placed, were to work them up, he would require from seventy to seventy-five years of hard work to bring out the results which the careful study of the different departments ought to yield. At the same time Prof. Agassiz observes that little that is new has been added by the Challenger Expedition to the deep-sea fauna as developed by the American and English Expeditions of 1866 and 1869. Reasoning from these premises, "we may safely say that while any new expeditions will undoubtedly clear up many of the points left doubtful by the Challenger, and may carry out special lines of investigation only partly sketched out, yet we can hardly expect them to do more than fill out the grand outlines laid down by the great English Expedition."

Preservation of Ice in the Sick-Room.—Dr. Gamgee, in the *Lancet*, suggests a good method of preserving ice in small quantity for a considerable time at the bedside of a sick person. His practice is to cut a piece of flannel about nine inches square, and secure it by ligature round the mouth of an ordinary tumbler, so as to leave a cup-shaped depression of flannel within the tumbler to about half its depth. In the flannel cup so constructed pieces of ice may be preserved many hours, all the longer if a piece of flannel from four to five inches square be used as a loose cover to the ice-cups. Cheap flannel, with comparatively open meshes, is preferable, as the water easily drains through it, and the ice is thus kept quite dry. When good flannel with close texture is employed, a small hole must be made in the bottom of the flannel cup, otherwise it holds the water, and facilitates the melting of the ice. In a room with a temperature of 60° Fahr., Dr. Gamgee made the following experiments with four tumblers, placing in each two ounces of ice broken into small pieces. In tumbler No. 1 the ice was loose. It had all melted in two hours and fifty-five minutes. In tumbler No. 2 the ice was suspended in the tumbler in a cup made, as above described, of good Welsh flannel. In five hours and a quarter the flannel cup was more than half filled with water, with some pieces of ice floating in it; in another hour and a quarter the flannel cup was nearly filled with water, and no ice remained. In tumbler No. 3 the ice was suspended in a flannel cup made in the same manner and of the same material as in No. 2; but in No. 3 a hole capable of admitting a quill pen had been made in the bottom of the flannel cup, with the effect of protracting the total liquefaction of the ice to a period of eight hours and three-quarters. In tumbler No. 4 the ice was placed in a flannel cup made, as above described, of cheap, open flannel, which allowed the water to drain through very readily. Ten hours and ten minutes elapsed before all this ice had melted.

Grote's Theory of the Peopling of America.—Prof. Grote's theory of the original peopling of America, as stated in recent papers, is that the original inhabitants came from Asia by way of the north during the latter part of the Miocene or earlier part of the Pliocene, and that this Tertiary population spread to the south along the mountainous backbone of the two Americas; that, on the advent of the Glacial epoch, the people then living in the extreme north were modified by the change in climate and were brought down by the ice and followed it back again to the arctic circle, and that the present representatives of glacial man are the Esquimaux. Through a study of migrations Prof. Grote comes to the conclusion that the ice must have acted as a barrier to further communication between the two continents of Asia and North America, and consequently that the civilizations of Central America and of the mound-builders are indigenous. Grote concludes that the theory of an accidental migration from Asia during the Quaternary cannot be supported in view of recently-ascertained facts. In a letter dated February 11, 1877, Captain E. L. Berthoud (of the School of Mines at Golden, Colorado), who has studied the geology and archæology of the West since 1859, writes that Grote's theory "solves many knotty points in the antiquities and

prehistoric vestiges of Colorado." Captain Berthoud believes, from his observations, that man existed in the Rocky Mountain region prior to the deposit of gold in the Colorado mountain-slopes, Creek, Bar, and Placer diggings, about latitude 39° 30′ to 41° north. Captain Berthoud has not only found flint tools and chips in the gold-bearing glacial drift, with remains of fossil elephants, but also in the drift of older date below this gold-bearing drift. Flint tools have been also found in company with estuary shells of not later age than older Pliocene as determined by Prof. Conrad.

The Decline of Savage Races.—Virchow, in an address upon the present position of anthropology, makes a few very just observations upon the subject of the decline of savage races in the presence of civilized man. Thus he remarks that we must not, in the case of an entirely isolated people, judge of their capacity for culture from the signs of it which exist. The extinction of uncultured races, he thinks, is rather to be ascribed to the barbarousness of Europeans, and to their incapacity to educate savages. There is no evidence that uncivilized races must become extinct—indeed, the contrary is proved by the history of Europeans themselves. If the civilized people of the present day are the product of a higher development, we cannot regard the possibility of such a development as a cause of the extinction of races in the same stage of culture once occupied by ourselves.

Estimation of Alcohol in a Watery Mixture.—Dr. Werner Siemens has designed an ingenious apparatus, by which a stream composed of alcohol and water, mixed in any proportion, is so measured that one train of counter-wheels records the volume of the mixture, while a second counter gives a true record of the amount of absolute alcohol contained in it. The principle is described as follows: The volume of liquid is passed through a revolving drum, divided into three compartments by radial divisions, and not dissimilar in appearance to an ordinary wet gas-meter. The revolutions of this drum produce a record of the total volume of passing liquid. The liquid on its way to the measuring-drum passes through a receiver containing a float of thin metal filled with proof-spirit, which float is partially supported by means of a carefully-adjusted spring, and its position determines that of a lever, the angular position of which causes the alcohol-counter to rotate more or less for every revolution of the measuring-drum. Thus, if water only passes through the apparatus, the lever stands at its lowest position, and then the rotative motion is not communicated to the alcohol-counter, and this motion is rendered strictly proportionate to the alcohol contained in the liquid, allowance being made in the instrument for the change of volume due to chemical affinity between the two liquids.

Preservation of Iron against Rust.—We find in *Van Nostrand's Engineering Magazine* an account of Dr. William H. Sterling's process for preventing the rusting of iron. The principle of this system, we are informed, consists in the saturation of the iron with a non-oxidizing or non-oxidizable substance while the iron is in a properly heated and expanded condition, produced by heating in a vacuum or in a simple chamber. One method of applying this system is given as follows by the inventor: "A vessel of iron, or any suitable material of sufficient strength, is made in the form and size best adapted to the shape and dimensions of the iron which is to be treated, with the lid so constructed that the vessel may be closed hermetically, and at the bottom suitable pipes are arranged for conveying steam and water alternately, for the purpose of heating and cooling the interior." Suitably connected with this vessel is a power-pump to produce the necessary pressure, also appliances for obtaining a vacuum. The iron is now heated to the desired degree and placed in the vessel, the top closed hermetically and superheated steam turned into the pipes at the bottom, to keep the metal at the required temperature; at the same time an atmospheric vacuum is produced by an ordinary air-pump connected with the chamber; the proper quantity of pure paraffine, having been also previously heated, is now let into this chamber and forced under pressure into

the interstices of the iron, saturating it. When the iron has remained under this liquid pressure a sufficient time, it is gradually cooled by turning cold water instead of steam into the pipes, the pressure being kept up, however, until the iron is cool.

Destruction of Birds by Telegraph-Wires.—It is the opinion of Dr. Elliott Coues that in the United States many hundred thousands of birds are yearly killed by telegraph-wires. To show that this estimate is not extravagant, he cites his own observation while journeying on horseback from Denver, Colorado, to Cheyenne, Wyoming, the road for a considerable part of the way coinciding with the line of the telegraph. The most abundant birds of that region at the time (October) were horned larks and Maccown's bunting. "Almost immediately upon riding by the telegraph-wire," writes Dr. Coues in the *American Naturalist*, "I noticed a dead lark; and as I passed several more in quick succession, my attention was aroused. The position of the dead birds enabled me to trace cause and effect before I actually witnessed a case of the killing. The bodies lay in every instance nearly or directly beneath the wire. A crippled bird was occasionally seen fluttering along the road. Becoming interested in the matter, I began to count, and desisted only after actually counting one hundred in the course of one hour's leisurely riding—representing perhaps a distance of three miles." During the hour he saw three birds strike the wire; of these one had a wing broken, and another was dying in convulsions.

Natural History on the Great Lakes.—Prof. Comstock, of Cornell University, proposes to organize an aquatic school of natural history for work during the summer along the shores of Lakes Erie, Huron, and Superior. A steamer is to be chartered for the use of the school, and inland excursions are to be made to the mining regions and other points of scientific interest. A strong corps of instructors for zoölogy, botany, geology, etc., will be engaged, and collections will be made illustrative of the work done in these various departments. A portion of the collection will be the property of the students, while the remainder will be disposed of to such local societies, colleges, and schools, as may desire to purchase them in advance by taking shares at $10 each. The terms for admission to the school are very reasonable, viz., not to exceed $125 for thirty days, and $3.50 for each additional day. This, however, does not include the expenses of inland trips: such trips will only be made by such pupils as desire to take part in them, and will be so arranged as to require the least possible expenditure.

Winter-Quarters in the Arctic Regions.—The ship Discovery, of last year's British Arctic Expedition, wintered in latitude 81° 40′ north, longitude 64° 30′ west, in a well-sheltered inlet directly opposite to the winter-quarters of the Polaris. Here she lay imbedded in the ice for ten months and a half. In preparation for the long winter, a layer of snow ten or twelve inches thick was laid on the deck, but as it was found not to bind, it was mixed with ashes and water, and soon made a good macadamized road. Then snow was piled up outside the ship about fifteen or twenty feet thick. This and the layer on deck kept the warmth in the ship, and the temperature in the lower deck ranged from 48° to 56°. Between April 26th and October 16th the ship's company shot thirty-two musk-oxen, thirty-six hares, six seals, and five eider-duck—about four months' rations of fresh meat. Captain Stevenson, commander of the Discovery, considers the long winter the most enjoyable time of the whole period spent in the arctic regions, the ship being very warm and comfortable, and all hands employed in the work most interesting to themselves.

Voice of the Elephant.—According to Major Leveson, author of "Sport in Many Lands," elephants utter four distinct sounds, each of which is indicative of a certain meaning. The first is a shrill whistling noise, produced by blowing through the trunk; this denotes satisfaction. The second is the note of alarm or surprise, a sound made by the mouth; it may be represented thus: *pr-rut, pr-rut!* The third is a trumpeting noise indicative of anger; when the

animals are very much enraged, or when they are charging an assailant, this sound changes into a hoarse roar or terrific scream. The fourth sound betokens dissatisfaction or distress; it is repeated frequently when an elephant is separated from the herd, or is tired, hungry, or overloaded; it may be thus imitated: *urmph, urmph.*

NOTES.

WRITTEN, as the little sketch of "Audubon's Flower" was, where access to books was impossible, and upon the memory of a reading of twenty years ago, I fear that, in the closing part, I may have overstated. It is not meant that Audubon *named* the flower, except conceptionally, or mentally, but that he did name it so far as a truthful bit of art could do, subordinated to a scientific conscience. S. L.

DR. LAWSON TAIT finds that, as a rule, the ear in women can perceive higher notes (i. e., sounds with a larger number of vibrations per second) than the ear in men. The highest limit of and ability for the human ear is somewhere between 41,000 and 42,000 vibrations per second. Very few of the persons experimented on by Dr. Tait had equal sensibility to acute sounds in both ears the right ear usually hearing a higher note than the left. The sense of direction of the sound in the human ear seems to be lost at a very much lower point than appreciation of the note. This, however, is not the case with cats.

ON January 11th died Mr. Alfred Smee, aged about sixty years. He was elected Fellow of the London Royal Society at the early age of twenty-one. Among his published works were the following: "Elements of Electro-Metallurgy," "Elements of Electro-Biology," "Monogenesis of Physical Forces," "The Mind of Man," etc.

KARL ERNST VON BAER, the eminent biologist, whose death occurred in November, was born in Esthonia, February 12, 1792. In 1819 he became Professor of Zoölogy in the Königsberg University. He was called to St. Petersburg in 1834, and was appointed librarian of the Academy. He led a scientific expedition to the northern shores of Russia in 1837. He wrote several works on zoölogy and botany, especially those of Northern Russia.

WILHELM F. B. HOFMEISTER, Professor of Botany in the University of Tübingen, and author of several works on plant physiology and embryology, died on January 12th, at the age of fifty-two years.

THE world of science has recently suffered another loss in the death of David Forbes, F. R. S., the geologist, at the early age of forty-eight years. He was a great traveler, and among his published papers may be named those on the "Relation of the Silurian and Metamorphic Rocks in the South of Norway," and on the "Geology of Bolivia and South Peru."

BLANCA PEAK, in Colorado, the elevation of which was determined last year by Hayden's survey, is probably the highest point within the limits of the United States. Its height is 14,464 feet above the level of the sea. There are in Colorado over fifty other peaks which rise more than 14,000 feet above sea-level.

MR. ROBERT E. C. STEARNS mentions, in the *American Naturalist*, two remarkable instances of vitality in snails. One snail, of the species *Bulimus pallidior*, lived for two years, two months, and sixteen days, without food, and at the end of that period appeared to be in pretty good health. Another, *Heliz Veatchii*, lived without food from 1859 till 1865. Both of these species of snails are indigenous to nearly rainless regions.

THERE is a pretty constant increase in the decennial number of plural childbirths in the kingdom of Prussia. In the period between 1824 and 1834 this class of births amounted to 112 per 10,000 births, and the same proportion was repeated in the succeeding decennium. From 1844 to 1854 the proportion was 114 to 10,000; from 1854 to 1864, 123; from 1864 to 1874, 128. Of these plural births, the immense majority, nearly 99 per cent., were twins. Triplets were somewhat less than 1 per cent. In over 6,000,000 births there were only 79 cases of four at a birth, and one case of five at a birth.

THE ATAMASCO LILY.—A new form of this favorite amaryllis, *A. Atamasco*, has been found in Florida by Mrs. Mary Treat. It is an earlier flower than the old form, and is larger and handsomer.

IT was stated by Mr. Sidebotham, at a meeting of the Manchester Literary and Philosophical Society, that aniline colors are now much used by artists both for paintings and water-color drawings. But, as nearly all of these colors fade under the action of light, no artist who wishes his work or fame to endure can afford to employ them.

YEARS distinguished by a maximum of sun-spots coincide very closely, according to Prof. Fritz, of Zürich, with years of extraordinary hail-fall, or unusual average height of the great rivers.

A SHORT time ago a number of fossil footprints, supposed to be human, were discovered in the carboniferous sandstone near Metropolis, Illinois. A physician living in that locality, Dr. Gebhart, took plaster casts of these footprints and sent a description of them, together with full details as to site, to Mr. Darwin and other naturalists. The almost unanimous verdict was, that the tracks were those of a species of *Labyrinthodon*. According to Dr. Gebhart, the animals which made these fossil tracks were most certainly bipeds.

IN countries where the coffee-tree is cultivated the leaves are used to make an infusion which by many persons is held to be superior to the infusion from the berry. Hitherto they have not been an article of commerce, and the planter has studied to obtain as large a crop as possible of the berry, neglecting the leaves. But if a demand for the latter should spring up in foreign countries, the planter would find it as profitable to cultivate the coffee-tree for its leaves as for its fruit. The berry would first be secured, with a sparing use of the pruning-knife, and then the leaves would be carefully gathered and cured for exportation. The result would be in a great measure to drive out of the market the spurious compounds that now too often are sold as coffee.

IT was in 1865 that the phylloxera appeared in the vineyards of the south of France; its ravages have been continued ever since. The department of Gard, which used to produce 126,000,000 gallons of wine, now yields not one-fourth as much. One commune, Castries, in the Department of Hérault, annually produced, before the appearance of the phylloxera, 3,000,000 gallons; one year later the product was 250,000 gallons; three years later the vineyards had been entirely destroyed!

A SCIENTIFIC journal of Paris notes the occurrence of a peculiar phase of insanity among French cooks. It is called *folie des cuisiniers* (cooks' insanity), and is due to the carbonic oxide given off by charcoal-stoves. The principal symptoms are hallucinations of sight and hearing, vertigo, oppression, and syncope. The patient generally believes himself to be the victim of persecution.

THE efficacy of the alkaline sulpho-carbonates as a means of exterminating the phylloxera appears to have been demonstrated by experiments made by Mouillefert, at the instance of the Paris Academy of Sciences. It still remains, however, to devise suitable methods of employing this insecticide. "Science," says M. Mouillefert, "has accomplished its mission, and it is now for agriculture to perform its part."

EARLY in the present year a State Zoological Society was organized in San Francisco, with the object of collecting material for a public museum of Pacific coast rocks, fossils, ores, and all inorganic substances having a bearing on practical geology. Another purpose of the society is to promote geological research. The coöperation of mine-owners and mining-engineers on the Pacific slope is solicited by the president of the society, so as to make the proposed collection fully representative of the geology of that portion of the United States.

BETWEEN Nice and Monaco is a locality so unhealthy that the Paris, Lyons & Mediterranean Railway Company have been obliged to change every two or three months the watchman at the crossing there. Plantations of the eucalyptus have been formed at this place, and at present the same watchman has resided there for several months with his family without experiencing the least inconvenience.

ON investigation, in Paris, of a case of lead-poisoning, no lead could be found in the cooking-utensils or in the food and drink of the patient. Lead was discovered, however, in a piece of a Roquefort cheese, which was enveloped in a metallic sheet, composed of 12 parts of tin, 85 of lead, and 3 of undefined matter. The conclusion drawn was, that the lead contained in the cheese was imparted to it by the envelope.

TRIALS have been made in Rome of a solution of chloride of calcium as a substitute for water in laying dust in streets. The results are said to be highly satisfactory. The dampness communicated to the road, instead of disappearing quickly, as is the case when water alone is used, remains for a whole week. The road continues to be damp without being muddy, and presents a hard surface, on which neither the wind nor the passing of pedestrians or horses has any effect.

SOME fifty years ago two gangs of workers in a Belgian coal-mine were at variance, and one party made a fire so as to smoke out the other. The coal in the mine became ignited, and it continues to burn down to the present day. Efforts have been made again and again to extinguish the fire, but in vain. Mr. Richard P. Rothwell, editor of the *Engineering and Mining Journal*, who mentions this case in a paper on fires in mines, cites a few similar instances from the history of mining in the United States of seams of coal burning for several years —as the Summit Hill Mine, near Mauch Chunk; the Greenwood Company's mine, near Tamaqua; and others in Schuylkill, Carbon, and adjoining counties of Pennsylvania. Some of these mines have been burning upward of twenty years.

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